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ART. I.—*On Cephalization, and On Megasthenes and Microsthenes, in Classification* (being in continuation of an article on the Higher Subdivisions in the Classification of Mammals); by JAMES D. DANA.

IN the paper on the Classification of Mammals, published by the writer in the last volume of this Journal (p. 65), and also in his earlier paper on Crustaceans, the principle of cephalization is shown to be exhibited among animals in the following ways:—

1. By a transfer of members from the *locomotive* to the *cephalic* series.

2. By the anterior of the locomotive organs participating to some extent in cephalic functions.

3. By increased abbreviation, concentration, compactness, and perfection of structure, in the parts and organs of the anterior portion of the body.

4. By increased abbreviation, condensation, and perfection of structure, in the posterior, or gastric and caudal, portion of the body: as, in the greater compactness and larger number of segments combined in the sacrum of the *higher Megasthenes* than in that of *Cetaceans*, or *Edentates;* the less posterior elongation of the vertebral column and body in the *higher Megasthenes* than in *Cetaceans*, or in the *tailless Batrachians* than in the *tailed* species of the group, etc.

5. By an upward rise in the cephalic end of the nervous system. This rise reaches its extreme limit in Man. Birds thus show their superiority to Reptiles: but not to Mammals; for the bird-type, like the Reptilian, is relatively diminutive in life-

system (p. 9, beyond); its relation to the Reptilian type is much like that of Insects to the Crustacean (p. 6).

A decline in the grade of cephalization is shown by the reverse of these conditions: as (1) by a transfer of members from the cephalic to the locomotive series; (2) by the posterior cephalic organs participating in locomotive functions; (3, 4) by increased laxness, length and breadth, or spacing, among the parts of either the anterior or posterior portion of the body; (5) by increased proneness in the position of the nervous system. Also—

6. By an adaptation of the organs of the senses to locomotive or prehensile purposes: as in the case of the proboscis of the Elephant, which is a perverted nose; also the prehensile terminations of the second antennæ of many inferior Crustaceans.

7. By an abnormal multiplication of the parts in the anterior portion of the body: as in the excessive number of teeth in some *Cetaceans* and *Edentates*.

8. By an abnormal multiplication of the parts in the posterior portion of the body: as in the abnormal multiplication of members and segments in *Phyllopod Crustaceans*, *Myriapods*, etc.

9. By a further degradation of the structure before and behind, or a degeneration or obsolescence of the parts or organs: as in the absence of teeth in some *Cetaceans* and *Edentates;* the degradation of feet into fins, as in *Whales*, or their total absence; the absence of a series of abdominal members in *Entomostracans;* the absence of antennæ in Articulates, *provided* the senses corresponding to these organs are absent or comparatively imperfect; the coalescence of the head and thorax, or of these with the abdomen; the extension towards, or into, the head of the gastric viscera.

10. By excessive size of body through mere vegetative enlargement: as in the *Megatherium*, the *female Bopyrus*, *Limulus*, etc.

Degradation, or a decline *below the normal* level, may hence be—

I. *Multiplicative.*—Methods 7, 8, above.

II. *Degenerative.*—Methods 3, 4, 9.

III. *Vegetative.*—Method 10. Also IV. *Phytoid* (or *plant-like*), when animals (as *Polyps*) have (11) the power of budding, or (12) a radiate structure, or (13) attachment below; and in such cases the decephalization is often almost as complete as in plants.[1]

Examples of *cephalization* by the first method, or by a transfer of members from the locomotive to the cephalic series, (or

[1] The methods of decephalization in Crustaceans are embraced under *two* heads by the writer, in his paper on the Classification of Crustaceans, (*this Jour.*, [2], xxii, 28, and *Expl. Exp. Rep. on Crustacea*, p. 1412,) as follows:—

"*First:* A diminution of centralization, leading to an enlargement of the circumference or sphere of growth at the expense of concentration, as in the elongation of the antennæ and a transfer of the maxillipeds to the foot-series, the elongation of the abdomen and abdominal appendages, etc.

"*Second:* A diminution of force as compared with the size of the structure, leading to an abbreviation or obsolescence of some circumferential organs, as the

of *decephalization*, by the reverse,) occur in the two highest subkingdoms, those of *Vertebrates* and *Articulates*. They fail in the two lower subkingdoms, those of *Mollusks* and *Radiates*, because of the absence of the necessary structure for showing it.

The examples under *Vertebrates* and *Articulates*, and the relations of the orders among *Mollusks*, may be briefly considered.

I. *Vertebrates.*—Only a single example in the class of Mammals, or even in the whole subkingdom of Vertebrates, is possible, owing to the fixed nature and simplicity of the head, and also the limited number of feet, two pairs being the maximum. This one example has already been pointed out and shown to be the basis of the grand distinction between Man and other Mammals. In passing downward from the exalted position which Man holds, there is a transfer of the fore-limbs to the locomotive series: the structure of the head in Vertebrates, even to the lowest Fishes, admits of no other case of analogous transfer.[2] In the *Walrus*, the tusks have some locomotive functions, as they serve to rest the fore-part of the animal or its head on the ice, while the body is in the water; but this is an example under the *second* method. The feet are wholly absent in *Snakes*, and the ribs aid in locomotion; but this is only a degradation of the vertebrate type, and not decephalization by the first method. In most *Fishes*, and in *Whales*, the locomotive function is transferred mainly to the elongated vertebrated posterior extremity of the body—a case of *degenerative* degradation, similar to the last, and analogous also to the *multiplicative*.

It is of sufficient interest in this connection to be repeated here, that among Mammals the four orders of Megasthenes exhibit in their *fore-limbs* four distinct grades of cephalization: in the *Quadrumanes*, these organs serve for carrying their young, supplying the mouth with food, taking their prey, and for locomotion; in the *Carnivores*, for taking their prey, and for locomotion; in the *Herbivores*, for locomotion only; in *Mutilates*, for fish-like locomotion, the members having the degraded form of fins.

II. *Articulates.*—In the subkingdom of Articulates, the three classes are *Insecteans*, *Crustaceans*, and *Worms:* the first includes

posterior thoracic legs or anterior antennæ, or the abdominal appendages (where such appendages exist in the secondary type embracing the species).

"These circumstances, moreover, are independent of a degradation of intelligence, by an extension of the sphere of growth beyond the proper limits of the sphere of activity."

[2] To the *zoological* characteristics of Man, mentioned in the writer's article on Mammals,—that is, the extreme cephalization of his system and the erect form connected therewith,—should be added the following, that, while in the Quadrumanes the feet are clasping or prehensile feet, in Man they are simply organs of support and locomotion. The former fit the Apes for their climbing habits, the latter, Man, for human duty. The discussion, now in progress, whether the hind-limbs of the Gorilla terminate in hands, or in true feet—"in no sense hands," in the words of Prof. Huxley—is of small importance in this connection.

The writer's view of the characteristics of Man depending on his *spiritual* nature are given in the last volume of this Journal, on page 452.

Air-breathing species, (*Insects*, *Spiders*, and *Myriapods*,) and the second and third, the Water-articulates. Examples of cephalization by the *first* method occur in the first two of these classes. They can not in the third, because Worms have no proper feet, and are not a type with *closed* limits, but one admitting of indefinite multiplication of parts behind and therefore *open* posteriorly.

1. *Insects*, the highest of the three orders of Insecteans, have three pairs of mouth-organs, and three pairs of legs. As the wings belong to the same segments of the body with two of the pairs of feet, they are not to be counted; for the transfer noted is, in fact, a transfer of *segments* of the body along with their appendages.

Passing down from Insects to *Spiders*, the mouth loses one pair of organs, the posterior, and the feet gain one pair, there being *four* pairs of feet in Spiders—that is, there is a transfer of *one* pair from the cephalic to the locomotive series. The absence of antennæ in Spiders is no mark of degradation, since the *senses* exist in good perfection.

Descending lower, to the *Myriapods*, the Articulate type passes below the range of normal variation into a degradational form, and one which, like that of *Worms*, admits of indefinite posterior elongation or multiplication of segments (by the *eighth* method of decephalization), and hence it has no *closed* or fixed limits, like that of Spiders or Insects. Under this loose and multiplicative condition of the system, there is no regular transfer backward of another pair of mouth-organs: the type is distinguished, instead, by the degradational character just mentioned.

2. The facts among *Crustaceans* have already been pointed out: that, descending from *Decapods*, (Crabs and Lobsters,) which have *six* pairs of mouth-organs and *five* of feet, to *Tetradecapods*, two pairs of the mouth-organs are transferred to the locomotive series, making the number of pairs of feet *seven*, and of mouth-organs *four*.

Descending further, to *Entomostracans*, or the third order, the mouth-organs lose one or more of the remaining pairs, and sometimes, as in *Limulus* (or the Horse-shoe Crab, as it is called) *all*, for the mouth-organs in this species are all true feet. The Entomostracans exemplify decephalization by degeneration (*ninth* method): as in the absence of one or two pairs of antennæ; the absence of one or two or more posterior pairs of thoracic feet; the absence of the series of abdominal members; and sometimes, as in *Limulus*, by the reduction of the abdomen to a mere spine. They are *degradational* forms, as well as the Myriapods; and, hence, the apparent difference of grade, which might be supposed to be marked by the number of pairs of mouth-organs transferred backward, can not serve to subdivide the order. The distinction of the Entomostracans from the higher

Crustaceans consists rather in their degradational characters than in any peculiarities of the mouth. In the tribe of Ostracoids (Cypris, etc.) alone, one genus has *two* pairs of mouth-organs, the rest being legs, another *three*, and another *four*, the Tetradecapod number.

III. *Mollusks.*—It has been remarked that the subkingdom of Mollusks cannot, from its nature, exemplify the *first* method of cephalization. The methods exemplified are the *third, fourth, ninth* and *tenth.* In the transition from the order of *Cephalopods*—the *first*—to that of *Cephalates* (*Gasteropods*),—the *second*—there is a loss of the feet or arms, and a diminished perfection of the senses, and activity is reduced to sluggishness. Descending to the *third* order, or *Acephals*, the antennæ fail, the eyes become imperfect or obsolete, locomotion becomes very imperfect, and in some fails altogether. Among *Bryozoans*, a still inferior order, all the organs of the senses fail, and there is the radiate structure of vegetation as well as its sessile character.

The difference in cephalization between an oyster and a clam is very strongly marked, the oyster, when placed in its normal position, having its body nearly all posterior to the beak, being merely a large gastric mass, and the clam having one-third of the body anterior to the beak, and really exhibiting something stately in mien compared with the oyster.

Other illustrations of the subject might be given; but they are not necessary to explain the general principle in view.

The *number of pairs of feet* in the subkingdoms of Vertebrates and Articulates, under those types which afford examples of the first method of cephalization, is as follows:

I. Vertebrates.

1, in Man; 2, in all other Vertebrates.

II. Articulates.

1. *Under Insecteans:* 3, in Insects; 4, in Spiders.
2. *Under Crustaceans:* 5, in Decapods; 7, in Tetradecapods.

The number of pairs of feet in the different groups are then 1, 2, 3, 4, 5, 7. Only *one* case of typical transfer occurs in each of the three classes illustrating the subject, Mammals, Insecteans, and Crustaceans; and these cases occur uniformly between the *two highest orders* of the class.

Man's title to the place assigned him in our former paper appears therefore to be unquestionable.

The types of Vertebrates and Articulates do not admit of any homological comparisons.

The types of Insecteans and Crustaceans are modifications of a common type; yet the two are so widely different, that it is far from true that the *five* pairs in the highest Crustaceans corres-

pond to the *four* in Spiders *plus* a preceding pair of mouth-organs. The head and locomotive part of the thorax in the Land-Articulates appear to correspond unitedly, as stated by Latreille, to the cephalic portion of the Crab, that is, to *nine* anterior segments out of the *fourteen* cephalo-thoracic. In other words, this part of the body of an Insect is an extreme concentration of the anterior portion of a Crustacean—an example of extreme cephalization; while a Crustacean is a diluted Insect, being much larger, and more numerous in segments and members.[3]

The *Lobster* (or any ordinary Macrural Decapod Crustacean) has an elongate body, and an abdomen well developed and furnished below with a full series of members. In the male *Crab*, also a Decapod, the body is very short, and the abdomen is without its members, besides being so small that it folds into a groove in the under shell of the body; this diminution of size and increased compactness are a consequence of the higher cephalization of the species (Method 4). Passing from Crabs to the still higher Articulates, *Insects*, there is an example of this cephalization carried to its maximum, it appearing in the extreme diminution of size of body and members, in the very small distinct head (comprising, normally, *a third* of the segments of the body, though so small), and in the thorax freed from the viscera and devoted mainly to locomotion. By this method, an animal is made of the highest instincts under the Articulate type.

From these examples it is evident that where there is a compacting of the body connected with rise in grade, it is not merely a *general* compacting of the different parts alike, or a general concentration and perfecting of the system, but a true *cephalization* of the system,—the compacting and perfecting showing itself primarily in a greater concentration, predominance, and domination of the cephalic extremity.

Among Articulates having feet, an *Insect* and a *Limulus* stand at the opposite poles of cephalization. The mouth-organs and feet in both correspond to those of the head (or the mouth-

[3] There appears to be no reason to doubt that, in all types, *not degradational*, each pair of members (wings excluded) corresponds to a separate normal segment of the body. Audouin & Edwards are sustained in their views on this point by the fact, that in a *Squilla*, three anterior cephalic segments (those of the eyes and two pairs of antennæ) and four posterior thoracic are actually distinct; and in an *Erichthus*, other segments, anterior to these four, are faintly indicated. (See the Author's *Expl. Exped. Report on Crustacea*, plate 41).

Assuming the number of normal segments anterior to the mouth in an Articulate from that (three) in the head of a Crustacean, the complete number in an Insect is *eighteen;* and in a Crustacean, *twenty-one, three abdominal* being present which are obsolete in an Insect. In the former, *half* (or *nine*) pertain to the head and thorax (only three to the thorax); in the latter, *two-thirds* (or *fourteen*); the rest being abdominal. In an Insect, the viscera are *abdominal*, in a Crustacean (excepting some degradational forms,) *thoracic*. The separation of the viscera from the thorax in an Insect leaves this part to higher purposes. It is to be noted, that the 10th to the 14th segments, inclusive, are *visceral segments in both Insects and Crabs*—being the first part of the abdomen in an Insect, and the last (and large foot-bearing) part of the cephalothorax in Crabs.

organs) of a Crab. But in *Limulus* there is extreme of degradation, all the members being large and stout feet, only the basal joints of the feet serving as jaws,—the body being enormously enlarged by mere vegetative growth,—the antennæ wanting, or reduced to a pair of pincers, and the animal sluggish, a sport of the waves on a beach; while in Insects, there is extreme of cephalization, the pairs of feet only *three* and those small and slender, and the body minute in comparison—the antennæ well developed and serving as delicate organs of sense—the animal active, and wonderful in its instinctive habits and knowledge.

The parallelism, above shown, between Insecteans and Crustaceans proves that *Insects*, *Spiders* and *Myriapods* are orders in a single class, and not separate classes.[4] Moreover, the orders under the classes of Insecteans and Crustaceans constitute parallel series, the first two of each being *closed* types, within the range of normal variation, and the last one of each (*Myriapods* and *Entomostracans*) being a degradational type, though different, one from the other, in kind of degradation. The parallelism between the series would be well exhibited if the orders were thus named:

Those of Insecteans, (1) *Hexapods*, (2) *Octopods*, (3) *Myriapods;*

Those of Crustaceans, (1) *Decapods*, (2) *Tetradecapods*, (3) *Colopods*, this last term (from κόλος and πους) signifying *defective feet* or *members*, which is the prominent characteristic of the order.

The parallelism extends even further than has been mentioned. The Tetradecapods are not an intermediate type between Decapods and Entomostracans: on the contrary, they lie quite out of the range of either. The Decapods, in their degradational species, pass almost into Entomostracan forms, and not into Tetradecapod forms. So among Insecteans, the Spiders have the same isolated position and defined limits. Insects, in their degradation, approximate to Myriapods, not to Spiders. In fact, Spiders stand more nearly between Insects and Crustaceans than between Insects and Myriapods.

There is, here, a cross affinity between Insecteans and Crustaceans which is of great interest. The relation of common *Spiders* to *Brachyural Decapods* or *Crabs* is seen, (1) in the general

[4] The grand distinction of the subdivision of *Insects* consists in their having three pairs of mouth-organs and three pairs of feet; of *Spiders*, in having two pairs of mouth-organs and four pairs of feet; of *Myriapods*, in having, through degradation, an indefinite number of segments and feet. Hence, to include *Spiders*, *Myriapods*, and the Hexapod group of *Pulices*, *Lepismæ*, *Pediculi*, and the like, in one division called *Aptera*, as done by some naturalists who adopt the general division of Insecteans, is a violation of all true affinities.

Professor Agassiz recognizes the same three classes of Articulates, as above, by the writer, and the same subdivisions, or orders, of Insecteans, but "from embryological data." The writer has not felt ready to deprive Spiders and Myriapods of their place in separate classes, coördinate with those of Insects, Crustaceans and Worms, (a common method among zoologists,) until recently, when the special application to these Articulates of the principle above explained occurred to him.

form or habit of body (some Crabs are called sea-spiders); and (2) in the coalescence of the thoracic and abdominal nervous ganglions into a single central thoracic ganglion. At the same time, the division of *Scorpions*, among Spiders, is correspondingly related to that of the *Macrural Decapods*, (1) in the body consisting of a series of segments; and (2) in the nervous ganglions being distinct, one to each abdominal segment. Moreover the maxillipeds are long and chelate, like the outer pair in some inferior Macrurans.

Again, the *Myriapods* are distantly related to the *Tetradecapods*, they being similar in their annulated structure, each segment having its pair of feet, and some species of the former (as those of *Glomeris*) even resembling the latter quite closely in form, articulation, and antennæ, and many of them having also the habit of some *Oniscidæ* (Tetradecapods) of rolling into a ball.

Thus, the *second* order of Insecteans is related, as regards form, to the *first* of Crustaceans; and the *third* of Insecteans, to the *second* of Crustaceans.

The earliest of Crustaceans, the *Trilobites*, one of the *comprehensive* types as styled by the writer, are, therefore, not only intermediate between Entomostracans and Tetradecapods, but also, in some respects, between these and the Myriapods. Moreover, like the latter, Trilobites are abnormal in the very large number of segments of which the body is composed; and sometimes, also, they present no distinction between the cephalothorax and abdomen.

The facts pointed out prove conclusively that Insecteans and Crustaceans constitute classes of equivalent value.

2. *Megasthenes and Microsthenes.*

The two grand divisions of typical brute Mammals, the *Megasthenes* and *Microsthenes*, are not separated by any very marked difference in type of structure; and still there is a profound fundamental difference between them,—that, to which the names refer. This is in contrast with the fact among Crustaceans, the megasthenic and microsthenic divisions of which (the *Decapods* and *Tetradecapods*) stand widely apart. But in the class of Crustaceans, the structure varies between remote extremes, while, in that of Mammals, there is a remarkable fixedness, or an extremely limited range of variation. Hence, in the distinctions of *Megasthenes* and *Microsthenes*, among Mammals, we cannot look for the marked diversity that subsists between *Decapods* and *Tetradecapods*, although the naturalness of the subdivisions is none the less real. The words *Megencephals* and *Microencephals* (signifying *large-brained* and *small-brained* Mammals) may better satisfy the desire for names expressing something tangible in the structure. Yet they do not appear to indicate the fundamental distinction between the groups. A general structural

characteristic may yet be detected corresponding to these megasthenic and microsthenic qualities; but even then, the distinctive idea of the subdivisions could hardly be better expressed than by the names proposed.

The parallelism between the Megasthenes and Microsthenes among Mammals and the Decapods and Tetradecapods among Crustaceans suggests, that if the subdivisions be called *orders* in the latter case, they should be so called in the former.

The distinction between Megasthenes and Microsthenes may, perhaps, become more intelligible, if we regard a living structure as a *life-system*, or, speaking dynamically, a *life-battery*. In order that such batteries may have a very wide range of size, two or more plans of construction, more or less different, appear to be requisite. With one plan, there is a certain magnitude which is that of most efficient action and power; and from this magnitude, there may be a series of larger and smaller sizes, reaching to the outer limits of normal perfection; and then, if these limits be passed in either direction, that is, either on the side of too great magnitude, or of too little, degradation in the structure and its powers begin to appear.

To carry the species through another range of sizes, with normal perfection of structure, another somewhat different plan is required. The Megasthenes represent one such plan, the Microsthenes another.

This idea is brought out by the writer in his chapter on the Classification of Crustaceans already referred to. He there says, speaking of the orders of Crustaceans, viz: Decapods, Tetradecapods, and Entomostracans:—

"I. Each type corresponds to a certain system of force more or less centralized in the organism, and is an expression of that force,—the higher degree being such as is fitted for the higher structures developed, the lower such as is fitted for structures of inferior grade and size. In other words, the life-system is of different orders for the different types, and the structures formed exhibit the extent of their spheres of action, being such as are adapted to use the force most effectively, in accordance with the end of the species.

"II. In a given type, as the first, for example, the same system may be of different dimensions, adapted to structures of different sizes. But the size in either direction for structures of efficient action is limited. To pass these limits, a life-system of another order is required. The Macroura, as they diminish in size, finally pass this limit, and the organisms (Mysidæ, for example) are no longer perfect in their members; an obsolescence of some parts begins to take place, and species of this small size are actually complete only when provided with the structure of a Tetradecapod.

"The extreme size of structure admitting of the highest efficient activity is generally three to six times lineally the average or mean typical size. Of these gigantic species, three or four times longer than the mean

type, there are examples among the Brachyura and Macroura, which have all the highest attributes of the species. There are also Amphipoda and Isopoda three inches in length, with full vigorous powers. Among Entomostraca, the Calanidæ, apparently the highest group, include species that are three lines long, or three times the length of the mean type.

"III. But the limit of efficient activity may be passed; and when so it is attended with a loss of active powers. The structure, as in the female Bopyrus and Lernæoids, and the Cirripeds, outgrows vegetatively the proper sphere of action of the system of force within. This result is especially found in sedentary species, as we have exemplified in our remarks on the Cirripeds.

"IV. Size is, therefore, an important element in the system of animal structures. As size diminishes, in all departments of animal life, the structure changes. To the human structure there is a limit; to the quadrupeds also, beyond which the structure is an impossibility; and the same seems to be the case among Crustacea. The Decapod, as the size diminishes, reaches the lowest limit; and then, to continue the range of size in species, another structure, the Tetradecapodan, is instituted; and as this last has also its limit, the Entomostracan is introduced to continue the gradation; and, as these end, the Rotatoria begin. Thus Crustacea are made to embrace species, from a length of nearly two feet (or two hundred and fifty lines) to that of a one-hundred-and-fiftieth of a line. These several types of structure among Crustacea do not graduate, as regards size, directly from one to another, but they constitute overlapping lines, as has been sufficiently shown."

While on this subject of life-batteries, the writer would suggest that the grand dynamical distinction between Mollusks and Articulates may be this:

A Mollusk corresponds to a *quantity-battery*, but one of very weak force; that is, it is analogous to a galvanic battery of *two or three small pairs, at the most.* This is indicated, (1) by the structure of the species, especially the absence of all articulations, the animal—a locomotive digestive system—being, as it were, in one simple bag; (2) by the number of ganglions limited to three; and (3) by the sluggishness of the animal.

An Articulate, on the contrary, corresponds to an *intensity-battery*, or is analogous to a galvanic battery of *many small pairs;* for (1) the body consists of many segments; (2) there are nearly as many nervous ganglions as segments (normally, as many); and (3) the animals in the more typical species have extreme rapidity of movement, and high instincts. The small number of ganglions in most Spiders is evidently due to a coalescence of several in the one central thoracic ganglion, as in Crabs.

In the highest Mollusks, the Cephalopods (Cuttle-fish, etc.), the Invertebrate *quantity-battery* reaches its greatest power.

Vertebrates also appear to correspond to a quantity-battery (as shown by the simplicity of the nervous system); but to one admitting of vastly greater power.

ART. II.—*Observations upon some of the Brachiopods, with reference to the genera Cryptonella, Centronella, Meristella, and allied forms;* by JAMES HALL. Abstract of a paper read before the Albany Institute, February 3d, 1863.[1] (Communicated by the author.)

(Concluded from vol. xxxv, page 406.)

IN the *Thirteenth Report on the State Cabinet* (p. 74, 1860), I proposed the Genus *Meristella*, to embrace certain species before included under the Genus *Merista*, and which were shown not to possess the peculiar shoe-lifter process, or transverse septum, characteristic of the latter genus. I remarked as follows: "Restricting, therefore, the signification of the genus *Merista* to such forms as were originally included by Prof. Suess under that name, it becomes necessary to designate those species of similar form, but without the peculiar appendage of the ventral valve, by another generic term; and I would therefore suggest the name of *Meristella*, proposed by me last year."[2]

After describing the genus, I cited as illustrations several species from the Lower Helderberg group; and gave figures of the exterior of *Meristella princeps* and *M. nasuta*, the latter species from the Upper Helderberg group.

In the same Report I described three other species of the genus, viz: *Meristella Haskinsi*, *M. Barrisi* and *M. Doris*, but without giving illustrations of these.

Since, on the one side, this genus has been claimed to be equivalent to *Athyris*, and, on the other, the same author has placed some of its species under a later created genus *Charionella*, it seems necessary to repeat some of the characters of the genus in this connexion.

Genus MERISTELLA Hall, 1860.—The genus includes *terebratuloid* or *Athyroid* forms which are ovoid, more or less elongate, sometimes elliptical in outline, and not unfrequently transverse or sub-circular; valves unequally convex, with or without a median fold and sinus, and this feature usually confined to the lower half of the shell. Ventral beak more or less closely incurved (when closely incurved apparently imperforate), terminated by an aperture, the lower side of which may be formed by the umbo of the dorsal valve, or by a deltidium. Area none.[3]

[1] From the Transactions of the Albany Institute, with some verbal corrections and the introduction of subsequent observations by the author.

[2] In the *Twelfth Report on the State Cabinet*, 1859, page 78, in referring *Atrypa naviformis* of vol. ii, *Pal. N. Y.* to *Merista*, I said: "This species, and some others of the Clinton and Niagara groups, differ somewhat from true *Meristæ;* and should these differences prove of generic importance, I propose for them the name *Meristella*."

[3] Those species with the ventral valve closely incurved are *apparently* imperforate, since no foramen is visible above the umbo of the dorsal valve. In the separated valves of these species, I have not seen any deltidium; an open triangular space exists above the points of the dental lamellæ, and this communicates with the open cavity of the valve.

Valves articulating by teeth and sockets. Surface smooth or marked by fine concentric lines of growth, not lamellose, and indistinct or obsolescent radiating striæ, which are usually more

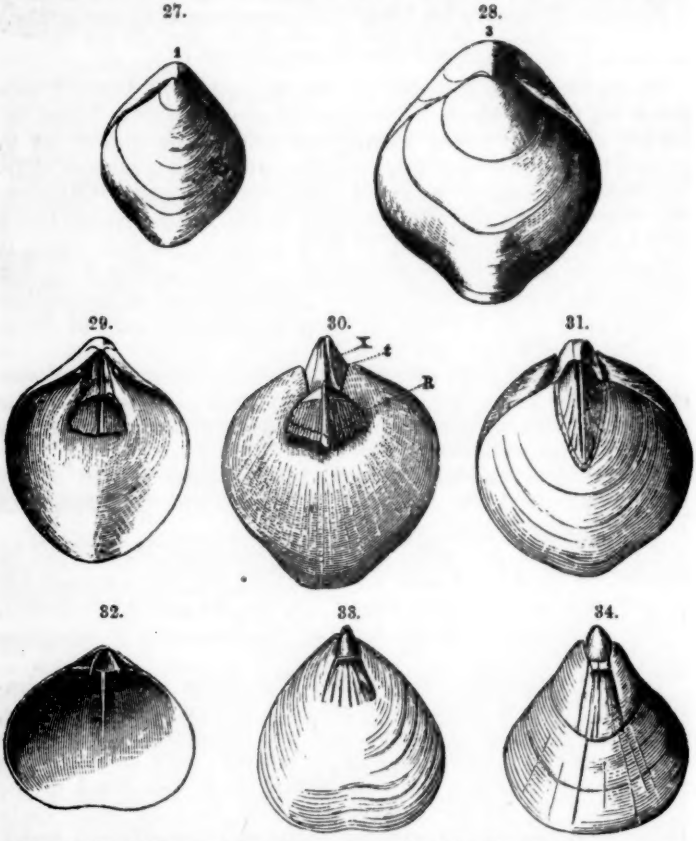


Fig. 27. *Meristella nasuta* = *Atrypa nasuta*, Conrad.—Dorsal view of a young individual.—Fig. 28. An older individual.—Fig. 29. Interior of the ventral valve.—Fig. 30. Cast of the ventral valve.—Fig. 31. Dorsal view of the same species.—Fig. 32. Interior of the dorsal valve of *M. arcuata*, showing the hinge plate and median septum.—Fig. 33. Cast of ventral valve of *M. Barrisi*.—Fig. 34. Cast of ventral valve of *M. Haskinsi*.[4]

conspicuous in the cast or exfoliated surfaces than on the exterior. Shell fibrous.

The ventral valve is much thickened on each side towards the beak, and the rostral cavity margined by flattened dental

[4] The casts of *M. Barrisi* and *M. Haskinsi* are obtained from solid specimens by removing the shells, and therefore have not that sharpness of the muscular markings which we find in weathered casts.

lamellæ, which extend downwards to the commencement of the muscular impression, and terminate at the edge of the shell in blunt tooth-like processes. The muscular impression forms a somewhat broadly triangular depression in the valve just below the rostral cavity. In the cast of this valve we have the reverse of these features.

In the dorsal valve there is a strong hinge plate or process, the prominent part of which is broadly triangular, somewhat depressed or spoon shaped in the centre, and supported below by a median septum which reaches from one-third to one-half the length of the valve, and on each side, marked by deep dental fossets, while the anterior angles are produced into the crura which support the internal spires.

Spires arranged as in *Athyris* and *Merista*, being a double cone with the apices directed outwards. From the lower lateral margins of the cardinal process or hinge plate, there is a callosity extending beneath and anterior to the dental fossets, and joining with the thickened margin of the valve, as in the other allied genera.

In the cast of the dorsal valve we have the mark of the median septum, with an elongate, lanceolate muscular impression, reaching nearly to the middle of the valve. The imprint of the triangular process, and the cavities made by the crura are often preserved.

The species of this genus may be readily distinguished from *Merista* by the absence of the shoe-lifter process, which, in numerous specimens compared, constitutes the principal difference between the two genera.[5]

The illustrations on the preceding page will serve to show more clearly the characteristics of this genus.

In the dorsal valve of *M. Barrisi* we have a hinge plate, with a median septum reaching more than one third the length of the shell, and the same characters exist in *M. Haskinsi.* In *M. Doris* the rostral cavity and muscular impression of the ventral valve are much elongated, and resemble what I have heretofore shown in *Meristella levis.*[6] The dorsal valve has a strong extended median septum and hinge structure as in the other species.[7]

The proportions of length of rostral cavity and muscular impression, vary in different species; and the muscular impression becomes much stronger and deeper in the older shells, when the valve as before remarked becomes thickened at the sides and

[5] On plates 39 and 41 of *Pal. N. Y.*, vol. iii, may be found some illustrations of the casts of species of this genus.

[6] *Palæontology of New York*, vol. iii, plate 39.

[7] In reclaiming these species of *Meristella*, I am not impugning the validity of the genus *Charionella* of Mr. Billings, for none of these have the characters of the dorsal valve of *Charionella* as represented on page 274, No. 33 of the *Canadian Journal*, which is not only clearly unlike *Meristella*, but very distinct from any genus of *Spiriferidæ* before described.

towards the beak. This character pertains to the limestone specimens, while those in the Hamilton shales, as figs. 7 and 8, have thinner shells, and less deep and strong muscular impressions.

I have already (*Thirteenth Report on the State Cabinet*, pp. 73–75, and illustrations on p. 93) pointed out the distinction between *Athyris*=*Spirigera* and *Meristella*. This difference is everywhere clear and unmistakable, in the external lamellose surface of the one, and the almost smooth character of the other. The muscular impressions of the ventral valve of *Athyris* are at once distinguishable from those of *Meristella;* as may be seen on comparison of figs. 35 and 36 with figs. 29 and 30.

35. 36.

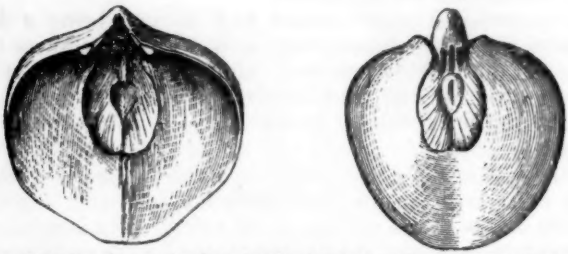


Fig. 35. Interior of ventral valve of *Athyris spiriferoides*.—Fig. 36. Cast of same.

In the dorsal valve, the muscular impressions differ from *Meristella;* the hinge plate is of somewhat different character, and the median septum is scarcely developed.

Note on the Genus LEPTOCŒLIA.—Among the specimens sent to me by Dr. Rominger, are two individuals of *Leptocœlia concava*, showing the existence of internal spires; and a careful examination of my own collections from the Lower Helderberg group has shown several specimens possessing these internal organs which have their apices directed obliquely outwards, and are connected near their origin by a strong vinculum on the dorsal side. After repeated examinations of a large number of the Oriskany sandstone species, from which the characters of the genus were mainly drawn, I have failed to detect internal spires. The form of the internal loop, as represented in the figures of the genus, was ascertained, as stated by me, mainly from cavities remaining in the crystalline filling of the shell. There were no appearances of spires, nor does a re-examination of the specimen afford any farther information, or indicate in any manner that spires have ever existed. The crura can be traced to the division at the process, and below this is a flat cavity.

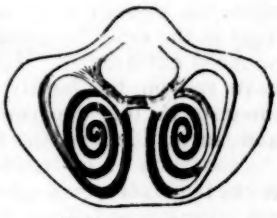
A critical re-examination of the fossils referred to this genus shows that there are at least three distinct types, in their external form and features, which, in the absence of positive knowledge of the internal structure, were grouped together. A far-

ther examination shows some peculiarities of hinge structure in each one of them, which are probably connected with more important difference of the internal parts. One of these types is indicated in the two strongly plicated species of the Oriskany sandstone, which have a median sinus near the front of the ventral valve, with two of the plications often closely incurved. Another type is that of *Leptocœlia concava*, and allied forms, which are more finely plicated, and where there is a sinus on the dorsal valve, though not distinctly defined. The third type is represented in *Leptocœlia* (*Atrypa*) *planoconvexa*, which has a somewhat undefined depression on the dorsal valve, and a form of cardinal process unlike the other species. The internal structure of this species is still unknown.

The *Leptocœlia imbricata* proves to be a *Trematospira*, and the same characters are apparent in *L. disparilis* of the Niagara group, the concavo-convex form of the shell being the only apparent deviation from typical forms of that genus. The *Terebratula lepida* of Goldfuss, as shown in the collections of Dr. Rominger, possesses internal spires precisely similar to those of *Trematospira camura*.

The *L. concava*, both in its external characters and in the arrangement of the crura and vinculum, differs from *Trematospira*, and with the knowledge at present possessed, I am compelled to separate this species from those last named, and from the *L. flabellites*, *L. fimbriata* and *L. acutiplicata*, I would propose to indicate forms of this external character with similar crura and spires as *Cœlospira*.

37.



Cœlospira concava.

The difficulty constantly attending the references of the Brachiopoda, to establish genera from external form and characters, renders it very desirable to search for the interior organization and appendages; but the condition of specimens does not always admit of satisfactory investigations, and not unfrequently the specimens possessed are so few as almost to preclude examinations of this kind.

As an example of the diversity of internal structure in similar external forms, I may mention the *Terebratula altidorsata* of Barrande, which so nearly resembles the *Centronella Glans-fagea* that it might readily be mistaken for that shell. On cutting and macerating specimens of the former, they prove to possess internal spires arranged as in *Meristella*, removing it from the family of the *Terebratulidæ*. I have not been able to determine whether the shell of this species is punctate or fibrous, from the specimens I possess.

ART. III.—*Hydraulics of the Report on the Mississippi River of Humphreys and Abbot;* by Prof. F. A. P. BARNARD.[1]

IN former notices[2] of this important Report, we have exhibited the general scope of the work, and presented some of its interesting details in regard to the physical geography of the great hydrographic basin to which it relates. We propose, in the present article, to examine the hydraulic investigations which occupy the greater portion of its bulk, and for the sake of which the laborious survey, of which it furnishes the history and the results, was originally instituted. We are induced to do this with some particularity, because, in the first place, the report itself is not so generally accessible as could be desired; and because, secondly, its conclusions, though novel and in some respects paradoxical, are yet so convincingly established upon the broadest possible basis of experimental proof, as undoubtedly to be destined to revolutionize the whole science of river-hydraulics, if in fact they may not properly be said to have, for the first time, created it. This may be considered strong language; and, whether regarded as an encomium upon the present work or a disparagement of what has been done in the same department heretofore, it will probably be pronounced extravagant. Lest, however, we should be thought to have spoken too lightly of the labors of former observers or theorists in this department of physical investigation, we cite here, in justification, the following sketch, by a master hand, of the condition of hydraulic science, theoretical and practical, as it existed sixty years ago; and as, without the slightest substantial improvement, it has continued to exist from that time to the present. The sketch is by the eminent Dr. Robison, of Edinburgh, himself a very felicitous, though not entirely an original, writer upon the same subject. Dr. Robison, after pointing out the extent to which the interests of every civilized people are involved in questions relating to the control and distribution of the running waters of the globe, and the great variety of ways in which we are constantly engaged in endeavoring to secure such control and distribution, proceeds as follows:

"Such having been our incessant occupations with moving waters, we should expect that, while the operative artists are continually furnishing facts and experiments, the man of speculative and scientific curiosity, excited by the importance of the subject, would, ere now, have made considerable progress in the science; and that the professional engineer would be daily acting from established principle, and be seldom disappointed in

[1] Report upon the Physics and Hydraulics of the Mississippi River; upon the Protection of the Alluvial Region against overflow; and upon the Deepening of the Mouths. By Capt. A. A. HUMPHREYS and Lieut. H. L. ABBOT. Submitted to the Bureau of Topographical Engineers, War Department, 1861. 4to, pp. 456 and cxlvi.

[2] This Journal, xxxiii, 181, xxxv, 223, 234.

his expectations. Unfortunately, the reverse of this is nearly the true state of the case: each engineer is obliged to collect the greatest part of his knowledge from his own experience, and by many dear bought lessons to direct his future operations, in which he still proceeds with anxiety and hesitation: for we have not yet acquired principles of theory, and experiments have not yet been collected and published by which an empirical practice might be safely formed. * * * The motion of waters has been really so little investigated, that hydraulics may still be called a new study." And again,

"As to the uniform course of the streams which water the face of the earth, and the maxims which will certainly regulate this agreeably to our wishes, we are in a manner totally ignorant. Who can pretend to say what is the velocity of a river, of which you tell him the breadth, the depth and the declivity? Who can say what swell will be produced in different parts of its course, if a dam or weir of given dimensions be made in it, or a bridge be thrown across it; or how much its waters will be raised, by turning another stream into it, or sunk, by taking off a branch to drive a mill? Who can say with confidence what must be the dimensions or slope of this branch in order to furnish the water that is wanted; or the dimensions and slope of a canal which shall effectually drain a fenny district? Who can say what form will cause or prevent the forming of elbows, the pooling of the bed, or the deposition of sands? Yet these are the most important questions. The causes of our ignorance are the want or uncertainty of our principles; the falsity of our theory, which is belied by experience; and the small number of proper observations or experiments, and difficulty of making such as shall be serviceable."

In asserting that this extract continues to represent the state of the science of river hydraulics at the present day, as completely as it did at the close of the last century, when it was written, we rest upon the notorious fact that these very questions, and every one of these questions, which are here set forth as unsolved in the time of the writer, are as far from finding their solutions in the works of the highest authorities on the subject, at the present time, as they were then. And though Dr. Robison himself proceeds to set forth a system—theoretic and practical—mainly adopted from Dubuat, which he concludes with affirming that he has "established," and which he assures us "may be confided in as a just representation of nature's procedure;" and though many eminent laborers, since him, in the same field, have put forth other systems, each believed to be an improvement on the last; yet nothing is better known than that the opinions and practice of our ablest engineers have been, and are yet, most widely at variance in regard to the simplest problems which present themselves relating to the control and management of our natural streams; or that, just in proportion as such streams are large, or are fed by numerous tributaries, drawing their waters from regions subject to diversified climatic vicissi-

tudes, in the same proportion they are defiant of all the dogmas of the books, and all the laws, however carefully elaborated, of hydraulic theorists. Indeed, in the very year in which the survey, of which we have in this report the results, was commenced, there was submitted to the Bureau of Topographical Engineers, at Washington, a report covering a portion of the same ground, viz: the question of the best means of preventing the overflows of the Delta of the Mississippi, by a gentleman reputed to be one of the ablest civil engineers the country has produced—the late Col. Ellet—in which all the received formulæ for determining one of the most important elements in the inquiry—the mean velocity of the stream—are set aside, and a new one introduced; and in which the conclusion is reached that protection by levees in the lower parts of the valley, is entirely impracticable. The latter view is one which many others, both before and since, have strongly held: and inasmuch as this river is a subject which has more or less occupied the mind of every man in the country having any pretensions to engineering skill, or any taste for this class of physical inquiries, it is a view which has been just as strongly discountenanced and as stoutly controverted, as it has been confidently maintained. Now if the science of river hydraulics had not been in the condition of uncertainty and imperfection which we have presumed to impute to it, how could it be possible that a great practical problem like this, the very foremost in magnitude of importance that could be stated in regard to the grandest of our rivers, could thus divide for years the opinions, not merely of the inexperts who dwell upon the banks of the stream and suffer from its ravages, but of the mathematicians and philosophers of the whole country, who exhaust, for its solution, all the resources of science; and of the practical men whose daily occupation it is to deal with precisely such cases as this, and who make the subjugation and control of unruly waters their profession!

The Mississippi has undoubtedly been, in this country, the standing opprobrium of hydraulic science. Lesser streams, in their occasional outbursts of disobedience, when they roar defiance at the artificial laws set up to govern their behavior, though they may embarrass and annoy, yet from their inferior force and volume, fail so utterly to astound the unhappy engineer whom they set at naught, as this monster of the Mississippi. Even blunders in hydraulic constructions may not glaringly betray themselves, when the force to be contended with is moderate. But the Mississippi will submit to no trifling; and the projector or the constructor who shall prescribe to it a rule which is not its own rule —that is to say, the rule of nature—will find his plans confounded or his works swept off in one wild ruin, the very first time the giant rises in his might. All this is so well known

that it has become habitual with the engineers, professional or amateur, whose notions and speculations this most independent of streams so constantly disconcerts, to shift the blame of the contradiction from their own shoulders to those of the river-god himself; and to speak disparagingly, perhaps we should say bitterly, of the Mississippi, as a lawless river, a capricious river, an inconsistent river, a congeries of anomalies which will never be understood, because, like Proteus, it never presents itself twice under the same aspect.

All this depreciatory and disrespectful language, is however sadly misapplied. The Mississippi is not a lawless river: it is only a large river. It is not a capricious river; but, draining as it does an immense hydrographic basin or system of basins, its hydraulic pulsations faithfully respond to every meteorological vicissitude in all that vast region, and present therefore phenomena which, at the time and place, may not always furnish their own immediate explanation. It is not an inconsistent river; for though it may sometimes seem, to the hydraulic engineer who studies its deportment, to conform itself with a docility truly gratifying to the formulæ which he has been taught to suppose should represent its movements, and at others may contradict them in a manner the most unceremonious and the most provoking; yet it is quite an error to draw, on that account, a conclusion injurious to the character of the stream. The true mode of looking at the phenomenon is this:—The formula is indeed inconsistent with the river, but not the river with itself. For the river being one of the forms of embodied nature, if the formula fails to represent it, then the formula is inconsistent with nature: and were the river to conform itself to the formula, it would be truly an inconsistent river.

It is probably in the fact that the Mississippi is a large river that we shall find a clew to the reason that it has been pronounced lawless. The laws by which it is presumed that flowing waters are governed, though partially deduced from theory, have been modified in their expression, by the accidental peculiarities of the minor streams whose habits they were designed to represent. Reduced to formulæ, they may exhibit no wide discordance with the phenomena of those particular streams, or of others which nearly resemble them. We by no means intend to deny to them this local or limited value. But these peculiarities may so mask the action of other and more general laws, that when the same formulæ are applied to the vast volumes of water which roll along the beds of the Ganges, the Amazon or the Mississippi, where the influence of such accidents bears to the great living force of the flood a ratio much less appreciable, they may be found to fail altogether. If it is true that flowing waters are governed in their movements by determinate laws,

which human observation is competent to detect, and which the human understanding is capable of comprehending, then certainly we may most reasonably hope to arrive at the discovery of those laws by attending to the examples in which their operations are illustrated upon the grandest scale. The Mississippi is therefore a river, which, so far from being lawless, is likely to afford us the most magnificent revelations of law: and its successful study is far more likely to afford us an intelligent understanding of the phenomena of lesser streams, than any amount of labor expended on such streams, to conduct us to a comprehension of the Mississippi. The correctness of this view is, we think, fully established by the Report before us; of which we will now attempt to present a succinct analysis.

The properly hydraulic portion of the Report commences with a concise exhibit of the existing state of hydraulic science, including an exhaustive catalogue of the writers who have treated of the subject. This is followed by an elaborate detail of the operations of the survey, both in the field and in the office, with a statement of the conclusions to which they successively led. Then succeeds an examination of the schemes which have been suggested for the protection of the alluvial lands of the valley against inundation; by straightening and shortening the channel of the river; by relieving it of a portion of its burthen of waters, through the diversion of tributaries or the creation of new outlets; and by confining the waters within levees or embankments higher than the highest floods. The first of these divisions we pass over. The third may form the subject of a future notice. It is the second which embraces the matter which interests us at present. In presenting the conclusions of the report as it regards the laws which govern the movements of flowing water, we shall depart from the order of the report itself so far as to group all these conclusions at once together, apart from, and in anticipation of, any explanation of the methods by which they are deduced. In this form their bearing upon each other, and their completeness as a system, will better appear. The authors, in their development of the subject, have strictly and very properly pursued the inductive method, by which the investigation was originally conducted; each conclusion being preceded by the full detail of the processes through which it was reached.

It must be premised that the problem of the flow of water in natural channels is complicated by many conditions subject to no definite law; such as the variations of cross-section, the number and magnitude of bends, and the irregularities and obstructions which may exist in the bed. In the first step toward the solution, the complications and uncertainties which the consideration of these particulars would introduce must be

avoided. The laws which we are about to state, are, therefore, to be understood of waters moving uniformly in channels straight and regular. With this explanation, we cite from the report, in a form condensed from that in which we find them, the following propositions:—

In a uniformly flowing stream, the maximum velocity of the water, in any vertical plane parallel to the current, is not found at the surface, but at a point situated a little more than three tenths of the depth below the surface.

To whatever cause it may be owing, there is a resistance to the flow of water at the surface, similar in kind to that which takes place at the bottom, though usually less in degree. This resistance is propagated downward, according to a law of diminution similar to that with which the resistance at the bottom is propagated upward.

If the velocities in the same vertical plane, parallel to the current, be plotted as ordinates, and the depths at which they are observed as abscissæ, the curve drawn through the points thus determined is sensibly a parabola, having the filament of maximum velocity for its axis, which is, of course, horizontal.

This parabola varies its curvature with the changes in the mean velocity of the river; the curvature being at its maximum when the velocity is greatest. When the velocity is zero, the parabola becomes a straight line.

The law which governs the curvature is determined by the proposition, that the reciprocal of the parameter of the parabola varies as the square root of the mean velocity of the river. The reciprocal of the parameter of sub-surface velocity is therefore the ordinate in another parabola, in which the mean velocity of the river is the abscissa.

The parameter of the parabola of parameters is, for rivers generally, sensibly constant. It is, however, a function of the depth; but such a function that the variations are nearly inappreciable for river formulæ, except for depths less than twenty or perhaps twelve feet.

The variations of velocity in horizontal planes, at the surface or below it, follow the same law as in vertical planes, the curve which represents them being a parabola having its axis in the thread of maximum velocity.

The depth of the axis of sub-surface velocities in vertical planes is affected by the wind, being depressed when the wind is up-stream, and elevated when the wind is down-stream. The amount of displacement is directly proportional to the force of the wind and the depth of the river, and is sensibly the same for the same wind-force, in either direction.

Neither the velocity at the surface nor the velocity at the bottom, nor, generally, the velocity at any determinate depth, is

a function of the mean velocity of the river only. The methods of gauging which have been founded on the assumption of a simple ratio, existing between some particular observed velocity and the mean velocity, are therefore all erroneous.

The ratio between the observed velocity, at any depth in any vertical plane, and the mean velocity, in the same vertical plane, is a function of three variables, which are the mean velocity of the stream, the depth of the river, and the force of the wind. There is one particular depth at which the ratio becomes independent of this last variable, and sensibly so of the depth of the river. This is the depth midway between surface and bottom.

The simplicity of the relation, between the mid-depth velocity and the mean velocity in the same vertical plane, suggests a method of gauging rivers, by which the labor of the process is greatly diminished, and its accuracy promoted. A method may be founded upon the observed velocity at *any* depth, provided the variables which affect the ratio between that and the mean velocity in the same plane are duly considered. Such a method was employed in the measurements of the Mississippi, during a considerable period of the operations of the survey.

The foregoing are the principal laws which govern the habitudes of water flowing uniformly in straight and regular channels. The following relate to the relations which exist between the cross-section, slope, and mean velocity of the stream.

The area of the cross-section, the wetted perimeter, the width, the slope, and the mean velocity of the river, are connected with each other by such relations, that, when the first three are ascertained by measurement, together with the discharge per second, the other two may be determined. For practical purposes, the wetted perimeter and the width may be treated as a single variable. The variables will then be four; and of these, if any two be given along with the discharge, unless the cross-section and mean velocity happen to be given together, the others may be found.

If a sensible addition be made to the waters of the river in any given stage, all the variables will be increased at once. The changes of the cross-section and the perimeter are, however, simple functions of the increase of depth and the inclination of the banks; or, the latter being disregarded as of trivial influence in a large river, of the increase of depth only. The change of slope is a function, but a less simple one, of the same unknown quantity. If, during a rise, the stand of the river above the extreme low water line be taken as the ordinate of a curve, and the increase of slope divided by the increase of depth be taken as the abscissa, the curve to which these coördinates correspond is a parabola. The parameter of this parabola is constant at the same locality, but its values at different localities are different.

By assuming for the change of stand, or the rise of the river, a hypothetical value, the new slope and new mean velocity may be computed; from which, in turn, a *calculated* value of the rise may be obtained. Thus, by a simple system of trial and error, the true value of the rise will easily be determined; and the resolution of involved equations avoided. In a similar manner, the depression of level, or the fall of the river, may be ascertained, in case any determinate portion of its waters be withdrawn by opening a new outlet.

The treatment of this problem by the authorities generally has the advantage over this of being greatly more simple; but it has also the disadvantage of not in the least representing nature.

These statements embrace, in brief, the substance of the contributions of this valuable report to the advancement of hydraulic science; and the basis of the new methods of practice which its authors have introduced. In estimating the effects of bends in the stream, they have adopted the principle of Dubuat, derived from observations on the flow of water in pipes, which makes the loss of living force proportional to the sum of the squares of the sines of the bending; the total amount of curvature being divided into angles below forty degrees. The agreement of the results of computation, upon this principle, with those of the observations instituted to test its correctness is very close, and is entirely satisfactory.

Whoever claims to have discovered a new law of nature, or a new truth in science, must expect the claim to be subjected to a severe scrutiny. This scrutiny will be directed equally to the processes by which he professes to have been led to it, and to the results which follow from its application in cases where deductions from it may be tested by direct observation, or by the degree of their accordance with other truths already known. This has been anticipated by the authors of this report, and they have accordingly furnished, in the amplest form, the material for applying either of the tests above suggested. We can only indicate in outline the nature of the material, referring those who would sift it thoroughly to the report itself.

Measurements of the daily discharge of the river were continuously made for periods of twelve months at Carrolton, Louisiana, of eleven months at Columbus, Kentucky, of ten months at Vicksburg, and one and a half months at Natchez. Similar observations were made upon the Arkansas at Napoleon, for eleven months; and, besides these, many others less protracted were made upon the main river and its tributaries and outlets, from the Ohio to the Gulf. These measurements required the determination of the cross-section and mean velocity at each station for every day. The cross-sections were determined by soundings

made at frequent intervals, in a line at right angles to the stream. The place of each sounding was fixed by observation with two theodolites, from the extremities of a base-line of from four hundred to one thousand feet in length, measured upon the bank. Two independent sections were sounded, two hundred feet apart; and soundings repeated on the same lines, at different intervals of time, showed that the bed underwent no sensible changes. Lines of level were also run up at the banks to points above the highest floods. From these data, with the daily gauge reading, showing the stand of the river, the cross-section was known for every day.

Velocities, both at the surface and beneath it, were ascertained by means of floats. In a river of such depth and power as the Mississippi, no kind of current-meter, or other contrivance involving mechanism, is available for sub-surface observations on velocity. The submerged floats were connected with surface floats very much smaller, by means of cords. The surface floats carried small flags which were observed in their transit across two lines at right angles to the river, two hundred feet apart, by means of theodolites at the extremities of a base of the same length on the shore. The point in which each float crossed each section line was thus fixed. As many observations as possible were made at all depths, and these were as equally distributed as possible throughout the breadth of the river. The paths of the floats were then plotted and grouped, by dividing the entire width of the river into spaces or divisions, each two hundred feet wide. The mean of the velocities of all the floats in each division was taken as representing the mean velocity of that division. For the shore divisions, when the floats were not well distributed through them, a slight correction was sometimes used. These mean velocities were then multiplied by the areas of their respective divisions of the cross-section; and the sum of the products was divided by the area of the entire cross-section, for the mean velocity of the river.

The process here described was employed in all the observations of 1851, the first year of the operations of the survey. When the work, after a long suspension, was resumed in 1858, the floats were all confined to the constant depth of five feet below the surface. The mean velocities at this depth, multiplied into their corresponding division areas, gave a result called "approximate discharge," which was reduced to the true discharge by being multipled into the ratio between the velocity at this depth, and the mean velocity in the entire vertical plane parallel to the current. This ratio had not been discovered while the earlier observations were in progress.

For the determination of the law of velocities below the surface, elaborate series of observations were made at Carrolton

and Baton Rouge by means of floats at different depths, from boats anchored in the stream at various distances from the shore. All the observed velocities of each set, that is, from each anchorage, were then plotted in curves. Subsequently, for the purpose of eliminating errors of observation and accidental irregularities, plots were made of the means of selected groups of observations, each group embracing the observations which corresponded to nearly equal depths and nearly equal velocities of the river. The resulting curves sufficiently indicated the existence of law, without clearly betraying its nature. Still another mode of combination of observations was resorted to. This consisted in forming a grand mean curve by taking the means of all the velocities at *proportional* instead of absolute depths—that is at every tenth, every two-tenths of depth, and so on. This was done by plotting the mean curves before obtained, on a scale which so exaggerated the differences of velocity, as to make one-thousandth of a foot an appreciable quantity, and then drawing through them parallel lines at each tenth of depth, taking the values corresponding to the points of intersection as the velocities due to those depths. Each point in this grand mean curve was fixed by two hundred and twenty-two observations: a number sufficient to obliterate, almost completely, the remaining irregularities. Its form suggests the probability that it may be one of the conic sections. In order to test this suspicion, and, in case of its truth, to determine to which of the conic sections the curve is to be referred, the general equation,

$$y^2 = 2Px + R^2x^2$$

is assumed; which is the equation of an ellipse when R^2 is negative, of a hyperbola when it is positive, and of a parabola when it is zero. If, in this equation, we give to x and y each two determinate values, represented by $x_{,}, y_{,}, x_{,,}, y_{,,}$ we may eliminate P, and obtain the value of R^2 in terms of $x_{,}, y_{,}, x_{,,}, y_{,,}$. As the equation assumes the origin of coördinates to be at the vertex of the curve, the plotted or tabulated velocities are not themselves the values of x required. Regarding the curve as a graphic representation of the condition of things in a vertical plane parallel to the thread of the current, it will be obvious that it must present its convexity down-stream; and that its vertex will be the point where a tangent is perpendicular to the horizon, the axis being in the line of maximum velocity. This is at about three-tenths of the depth below the surface. The abscissas will then be the differences between the maximum velocity and the velocities at other points of the curve; and the ordinates will be the distances of those points from the axis, in decimals of depth of the river. The values of R^2 as computed

from these data for every tenth of depth, though never absolutely zero, are always small, and are positive in a part of the curve and negative in the rest. A mathematically regular curve was not to be expected; but the approach to a parabola is so near as to warrant the conclusion that this is the curve according to which the differences of velocity are regulated, and according to which, therefore, the resistances are distributed through the moving mass.

The equation of the parabola is easily deduced. In the equation of the common parabola, if we assume particular values, $x_{,,}$ $y_{,,}$ for the coördinates, we shall obtain the expression,

$$x = \frac{x_{,,}}{y_{,,}^2} y^2;$$

and if an arbitrary increase, $= x_{,}$ be given to all the abscissas, this will become

$$x - x_{,} = \frac{x_{,,} - x_{,}}{y_{,,}^2} y^2; \quad \text{or} \quad x = \frac{x_{,,} - x_{,}}{y_{,,}^2} y^2 + x_{,}.$$

In applying this equation to the observations, $x_{,}$ is to be replaced by the maximum velocity, and $x_{,,}$ and $y_{,,}$ by the values of those coördinates at the points most distant from the axis. The values of x are then to be computed for all the depths at which actual observations of velocity have been made. The sums of the computed and observed values are then to be compared, and their difference, if any, divided by the number of points observed, is to be applied as a correction to $x_{,}$ and to all the values of x: an operation which amounts to moving the whole curve slightly along the axis, without altering its curvature. By varying the depth of the axis, or the position of the point $x_{,,}$ $y_{,,}$, it may easily be found where the closest accordance between the observations and the computations can be secured.

The equation of the grand mean curve of subsurface velocities having been obtained by the processes here described, the degree of its accuracy may be tested by comparing severally the values of the velocities computed by means of it, for all the points beneath the surface at which velocities were actually observed, with the tabulated mean observed values. The comparison as made furnishes the following results: The actual maximum velocity observed being in feet 3·2611, the greatest difference found between any computed and any observed mean value is only ·0066, and the least is ·0006. The sum of all the differences, taken without regard to sign, is only ·0245, which is less than three-tenths of an inch. This test is certainly very satisfactory; but it is corroborated by others to be presently mentioned.

The forms of the parabolas of subsurface velocities at high and low water, and at intermediate stages of the river, indicated a change of curvature consequent upon a change of mean velo-

city. The next object was to determine the law of change. The data which presented themselves were the high water mean, the low water mean, and the grand mean curves; to which another might be added by considering that if the mean velocity of the river be made zero, the parabola becomes a straight line. An attempt was made to investigate the relation of the parameters of these curves; but the data proved to be too limited for the purpose. The idea was then conceived of attempting the same investigation in regard to the velocities in *horizontal* planes; or at the surface of the river. Data for this inquiry had been amply furnished by the observations for daily discharge. These observations, which were made at a depth of five feet below the surface, were grouped according to the even feet of approximate mean velocity of the river; and thus were obtained material for eight mean curves, corresponding to as many different mean velocities. From these was deduced a grand mean curve, as in the case of subsurface velocities. The result was a very clear disclosure of the parabolic law.

In proceeding to the study of the law of variation of curvature, equations were deduced for each of the eight mean curves. The reciprocals of the parameters of these parabolas were plotted as ordinates, the corresponding mean velocities of the river being the abscissæ—the reciprocal of the parameter of the limiting parabola, or straight line, which is zero, indicating that the curve intersects the axis of abscissæ at the origin of coördinates. A curve resulted which conformed closely to the parabolic law; and thus furnished a general expression for the reciprocal of the parameter of any parabola of surface velocities corresponding to any given mean velocity of the river. This result was tested by applying it to the formation of a general equation for the curve of velocities five feet below the surface; and employing this equation to recompute the velocities corresponding to the eight mean curves which had been used as components in forming the grand mean. The differences were all small, though larger than those in the grand mean subsurface curve; a consequence probably of the fact that each of these eight curves was founded upon a much more limited series of observations than that. A law of parameters having been thus deduced for the horizontal curves, the probability naturally suggested itself that a similar law governs those of the vertical curves also. Materials which are insufficient to reveal the existence of an unknown law are often ample for the purpose of testing the fact of its existence, after it has been once suspected. We have seen that the only materials for a curve of parameters for subsurface velocities consisted of four pairs of coördinates. Of these the parameter and mean velocity of the grand mean curve were the pair best determined, next to those of the straight line, which

fixed the intersection of the curve with its axis at the origin of coördinates. A parabola, passed through the two points thus ascertained, furnished means for constructing a general equation of subsurface velocities, similar to the general equation of surface velocities before formed. The general expression for the reciprocal of the parameter, in such an equation, is, of course, the ordinate of the parameter curve corresponding to the variable mean velocity of the river (v). Or, putting b for the parameter of the curve of parameters, and $\frac{1}{2P}$ for the ordinate,

$$\left(\frac{1}{2P}\right)^2 = bv\,; \quad \text{or} \quad \frac{1}{2P} = \pm(bv)^{\frac{1}{2}}$$

The value of b deduced from the equations of subsurface velocities, was 0·1856.

Referring to the equation of the parabola before given, as adapted to the case in which the curve intersects the axis at a point of which the coördinates are $x=x_{,}$, $y=0$, viz:

$$x = x_{,} + \frac{x_{,,} - x_{,}}{y_{,,}{}^2} y^2,$$

it will be seen that this value of $\frac{1}{2P}$ is the coëfficient of y^2, or

$$\frac{x_{,,} - x_{,}}{y_{,,}{}^2} = -(bv)^{\frac{1}{2}},$$

the negative value being required by the nature of the case, since $x_{,}$ is the maximum velocity in the vertical curve. Putting then V for the general value of the velocity in the subsurface curve, and $V_{d,}$ for the particular velocity at the depth $d_{,,}$ which denotes the depth of the axis, the general equation of the subsurface velocities is

$$V = V_{d,} - (bv)^{\frac{1}{2}} d_{,,}{}^2,$$

where $d_{,,}$ denotes the distance of the point whose velocity is V from the axis, expressed in decimals of the depth taken as unity.

This equation was subjected to a very thorough testing, by being applied to the mean high water and low water curves, and also to other series of observations not included in the formation of those curves, and to observations on the bayous of La Fourche and Plaquemine. In the mean high water curve, the mean difference between the observed and computed velocities, expressed in decimals of a foot, was ·0074, the maximum velocity in the curve being 3·8371 feet. In the mean low water curve, the same mean difference amounted to ·0127, the maximum velocity being 2·2523. This curve was not so well determined as the other, yet the mean difference is not a sixth of an inch. In

medium-stage curves, deduced from 52 observations at Columbus and 20 at Vicksburg, the mean differences were ·0056 and ·0155 respectively; the maximum velocities in the curve being 4·1958 and 4·5709. In bayou Plaquemine the mean difference was ·036 on a maximum velocity of 6·491; and in bayou La Fourche ·003 on a maximum velocity of 3·250. In these two cases, the curves are deduced from the results of a single day's observation.

The same equation was also further tested by being applied to the original curves, which had been combined, as above stated, on the principle of *proportional* depths. The correctness of that principle of combination had not been quite certain; but the results of this final test left no doubt of its legitimacy. The mean discrepancy between the computed and observed velocities amounted in only one case to so much as one per cent of the maximum velocity in the curve; and in this instance, the absolute discrepancy was but a little over an inch. Usually the agreement was much nearer.

But a still more remarkable test of the accuracy of the law of parabolic velocities is furnished by a comparison of its results with observations made by Capt. Boileau at Metz, upon the flow of water in an open wooden trough only about two feet wide and one foot deep—the depth having been subsequently reduced to two-thirds of a foot. In these cases, the velocities observed in the vertical curve varied, in the first instance, from below two feet to nearly three; and in the second, from about one and a half to over two feet. The observations are recorded in the one case at fifteen different depths, and in the other at thirteen. The mean discrepancy, between these observations and the results of computation for the same points from the parabolic equation founded on them, amounted, in decimals of a foot, to only ·0330 and ·0243 for the two cases respectively. The largest of these mean errors is less than four-tenths of an inch.

It will be noticed that in all the computations made for the subsurface velocity of the Mississippi, in its different stages, and for the bayous, one and the same equation was constantly employed, viz:

$$V = V_{d'} - (bv)^{\frac{1}{2}} d_{''}^{\,2} = V_{d'} - (0{\cdot}1856v)^{\frac{1}{2}} d_{''}^{\,2};$$

the value of $V_{d'}$ being, in each case, taken directly from the observations, and v being known by the measurements of discharge. In this proceeding it was evidently assumed that b is a constant, and always equal to 0·1856. The equations derived from Boileau's trough furnished data, not quite exact but nearly so, for obtaining a new value of b. One of the necessary data was wanting, which was the value of v, the mean velocity of the stream, but as this will not be very far in error if taken at eight-tenths of the velocity on the surface, it may be assumed to be

well enough known for the purpose in hand. The numerical coëfficient of $d_{//}^{\frac{1}{2}}$, in the equation of the parabola, being then divided by the so-assumed value of $v^{\frac{1}{2}}$, will give the square root of the value of b which is sought. This value is found from the equation of the trough to be a little above unity. In the equation employed in the previous computations, being that which had been derived from the grand mean curve of the Mississippi, the numerical value of b, as we have seen, was 0·1856. It appeared probable, therefore, that this quantity, that is, the parameter of the parabola of parameters, varies inversely as some function of the depth. For the sake of further testing the truth of this supposition, careful observations were made upon a feeder of the Chesapeake and Ohio Canal near Washington, having a depth of 7·1 feet and a width of 23; being, in these dimensions, much smaller than the river, and much larger than the trough. The mean maximum velocity observed was a little over two feet and a half, and the mean difference between the computed velocities for the several points observed and the means of the observations themselves at those points was less than a quarter of an inch. The equation of the parabola deduced from the observations gave a value of b equal to 0·58. The value of b (or of the parameter of the curve of parameters) changes, therefore, very slowly at considerable depths; and is practically at its minimum value for rivers when it equals 0·1856. The following expression is given as representing the observations:

$$b = \frac{1{\cdot}69}{(D+1{\cdot}5)^{\frac{1}{2}}},$$

in which D denotes the depth of the river.

It being once established, that the curve of velocities in the vertical plane parallel to the stream is a parabola, and the equation of the parabola being known, the *mean velocity in the whole vertical curve* is easily deduced. The area representing the sum of all the velocities is made up of a rectangle and a parabolic segment above the axis, and of another rectangle and parabolic segment below. Dividing the sum of these areas by the total depth will give the mean velocity required. But the dividend in this case is an expression necessarily involving the depth of the axis as an element; and, although this mean depth had been found to be nearly constant, the actual depth is observed to vary. This variation is apparently dependent on the direction and force of the wind.

In the investigation of the effects of wind-force, the selected observations were divided into three classes—those in which the wind blew up-stream, those in which it blew down-stream, and

those when it either did not blow at all or blew directly across the stream. The wind-forces were estimated according to the usual scale of notation, 0 being a calm and 10 a hurricane. For the first two classes of observations, the sum of the products of the numbers of observations at each point by the force of the wind was made out for each, and the difference between the two sums, divided by the total number of observations at all the points, was presumed to give the effective force of the wind. Five sets of determinations were thus obtained, in each of which the number of observations, the depth of the axis, the mean velocity of the river, and the resultant force and direction of the wind were given.

If x be the unknown depth of the axis due to a calm, and d, $d_{,}$, $d_{,,}$, &c., the observed depths, and if y denote the amount of movement of elevation or depression of the axis which would be produced by a wind of force 1, we may presume that for movements not greater than y the changes will be sensibly proportional to the force. Also, the weight of the several wind determinations will be proportional to the number of observations from which they are deduced. Finally, the legitimate effect of an up-stream wind will be to depress the axis, and of a down-stream wind to raise it. Let the resultant wind-forces among the data be denoted by $f, f_{,}, f_{,,}$, &c., then the movements of the axis will be fy, $f_{,}y$, $f_{,,}y$, &c. Accordingly, we shall have

$$x \pm fy = d,\ x \pm f_{,}y = d_{,},\ x \pm f_{,,}y = d_{,,},\ \&c.,$$

the negative sign being used when the wind is down-stream, and the positive, when it is up. Multiplying both members of each of these equations by the number of observations from which its constants were deduced, and adding the whole, member for member, we obtain one equation containing both x and y. But if we consider that the sum of all the movements of the axis, taken without regard to sign, must be equal to the sum of all the differences between x and d, x and $d_{,}$, &c., taken positively—that is, taking $x-d$ when x is the greater, and $d-x$ when d is the greater, we shall, by multiplying once more all these differences and the corresponding movements (fy, $f_{,}y$, &c.) by the numbers of observations to which they respectively belong, and adding the products as before, obtain a second equation containing both x and y. From these equations combined, both x and y are determined. The deduced value of x is ·317 which is the depth of the axis when the wind force is zero, in decimals of the total depth of the river. The deduced value of y serves to reduce the observed depths of axis to the position of calm. A comparison of the values of x so obtained (which should all agree with each other, and with the value of x given by the

equation) verifies the general correctness of the procedure; the differences found being all very slight.

In these cases, the outstanding force of wind, after balancing opposing forces, was never much above 1. Velocity observations had, moreover, been found to be impracticable, with a wind above 4. The results of the investigation, so far, would hardly therefore justify the assumption that the movement of the axis is proportional to the force, for all degrees of force. Data necessary to set at rest the doubt which here arose seemed to be wanting. A happy expedient however presented itself for the removal of the difficulty. The daily "approximate discharges" of the river had been computed for each day's observations at Columbus, Vicksburg and Natchez, by taking the sum of the products of the several division areas by the velocity observed in each, five feet below the surface. These discharges, if accurately determined, would undoubtedly vary continuously; and if plotted as ordinates to a curve of which the dates are abscissas, should present a smooth and regularly varying curvature. But if the velocity five feet below the surface is affected by the wind, we may expect to see serratures in the curve, forming prominences when the wind is down-stream, and depressions when it is up. By examining the curve at the points of serrature, the amount in cubic feet of discharge may be estimated, which, by being added or subtracted, would restore the regularity of the curve. From these data and the wind record, it is easy to see that the amount of effect on discharge due to winds of different determinate forces, as 1, 2, 3, &c., may be deduced. This amount is found to vary with the mean velocity of the river.

In applying the principle just indicated to the determination of the effect, in raising or depressing the axis, of winds of different degrees of force, observations were selected of days when the wind record showed the several forces, 1, 2, 3, and 4, up or down the river. For these days a mean cross-section, a mean approximate discharge, a mean depth or *radius* of the river, a mean approximate mean velocity and a mean velocity five feet below the surface, were computed; and also the corresponding empirical correction of discharge was deduced from the observations of the same days. The unbalanced wind force was determined in the same manner as in the former investigation.

From the mode in which the approximate discharge is computed, it is evident that—the cross-section remaining the same—this discharge may be taken as proportional to the mean velocity five feet below the surface. Hence, if the originally computed or recorded approximate discharge be increased or diminished by the product of the unbalanced force of wind into the empirical correction corresponding to that force, the sum or difference will be the approximate discharge due to a calm, and may be called

the *corrected* discharge. The truth of the following proposition will then be manifest:—As the recorded discharge is to the corrected discharge, so is the observed mean velocity, five feet below the surface, to the velocity which would have been observed at the same depth had it been calm. But, had it been calm, the axis would have been at the depth ·317. In the general equation of velocities, therefore,

$$V = V_{d_I} - (bv)^{\frac{1}{2}} d_{II}^2,$$

if d_{II} be put for the distance from the known position of the axis to the point five feet below the surface, and the velocity found for that point by the last proportion be put for V, there will remain only one unknown quantity, which is V_{d_I}, or the maximum velocity in the vertical curve. This velocity is therefore easily deduced, and, being substituted in the same equation, will enable us to compute the *mean* velocity in the entire vertical plane. For this mean velocity, being derived from the areas of the rectangles and parabolic segments, above and below the axis, which form the figure bounded by the curve of velocities at one end and by a vertical line at the other, the extreme length being the maximum velocity in the plane, and its breadth the depth of the river, is determined when the velocities at surface and bottom are given along with the maximum velocity and depth. The last named velocity is that which was just found; and, by the help of this, the equation gives the other two, when the proper values of d_{II}, viz., distance from the axis to the surface and distance from the axis to the bottom, are substituted.

This operation was performed for each of the wind-forces, 1, 2, 3, and 4. In each case, the unbalanced wind-force of the observations was but a fraction of the total force of the wind at the given intensity. It was desirable to know the effect due to the entire force; and, in order to arrive at this, it was only necessary to reverse the operation. Thus, taking the corrected approximate discharge as the discharge due to a calm, and increasing and diminishing it by the amount of the empirical correction corresponding to each wind-force successively, we may obtain the hypothetical discharge due to the full wind-force in question, down or up the stream. Then the proportion may be stated:—As the corrected discharge is to the hypothetical discharge, so is the velocity in calm, five feet below the surface, to the velocity at the same point under the assumed wind force. The value so determined may be substituted for V as before, or for U on the left of the more general equation following; which differs from the former only in replacing V, the velocity in a particular plane, by U which is intended to denote the velocity in the mean of all planes parallel to the axis of the river,

$$U = U_{d_I} - (bv)^{\frac{1}{2}} d_{II}^2 = U_{d_I} - (0{\cdot}1856v)^{\frac{1}{2}} d_{II}^2.$$

In this equation, U_{d_i} and $d_{i'}$ are both unknown. The equation for *mean velocity in the vertical plane* contains (as above stated) values of U at the surface and at the bottom, both of which are expressible, from the equation just given, in terms containing only the same two unknowns, U_{d_i} and $d_{i'}$.[2] There will therefore be two independent equations, involving only two unknowns, and from these the depth of the axis, on which the values of $d_{i'}$ depend, may be determined.

This determination having been made for the four wind-forces independently, the amount of displacement of the axis due to each wind-force is ascertained. The result is, that the displacement is directly proportioned to the wind-force, that it is equal for the same wind-force in opposite directions, but that it is a depression for an up-stream wind and an elevation for a down-stream wind. It also appears that the amount of displacement is independent of the mean velocity of the river. And although, in this investigation, the data for determining the effect of each wind-force are entirely independent of those for the others, yet the results exhibit a remarkably close agreement. As a result of the whole we obtain the following formula which is a general expression for the depth of the axis, (denoted by $d_{i'}$) f representing the force of the wind, and r the radius, or mean depth of the river:—

$$d_{i'} = (0{\cdot}317 + 0{\cdot}06f)r.$$

Since the process just described furnishes the means of expressing the velocity at the surface or at any depth below it, in terms containing all the variables which affect its value, it is manifestly practicable to deduce a system of gauging a river in which the true discharge shall be obtained from observations at one unvarying depth. But, as the ratio of the so observed velocity to the mean velocity is a varying one, no system of gauging founded on this method of observation can be relied on, in which account is not taken of this variation.

There is, however, one velocity—the velocity at mid-depth—which bears a ratio to the mean velocity in the vertical plane which is nearly constant. In order to show the evidence of this, we must actually state the equation for this mean velocity to which we have several times referred. It will be necessary therefore to adopt the following symbols:

[2] The symbol $d_{i'}$ has, apparently, two distinct values as employed above:—it being used to stand both for the distance from the axis to the surface, and for the distance from the axis to the bottom. But, as the depth is known, these are simple functions of a single unknown quantity. We have endeavored, in the explanation of the principles of the report, to avoid the introduction of many symbols. We have not therefore, thus far, actually stated the equation for *mean velocity in the vertical plane* above referred to. The system of symbols employed in the report is very clear and expressive; but we have chosen to defer an account of it, until after completing our outline of principles and processes.

D=depth of river. d=distance below the surface (variable).
d_l=depth of axis (line of maximum velocity in the vertical plane).
m=depth of line of mean velocity in the vertical plane.
V=velocity at any point in the vertical plane.
V_{d_l}=velocity at the axis, or maximum velocity.
V_m=mean velocity in the vertical plane.
V_0=velocity at the surface. V_D=velocity at the bottom.

Then $V_m D$ will be the value of an area equal to the two rectangles and two parabolic segments concerned in the determination of the mean velocity. The truth of the following equation is therefore manifest:—

$$V_m D = \tfrac{2}{3}(V_{d_l} - V_0)d_l + V_0 d_l + \tfrac{2}{3}(V_{d_l} - V_D)(D - d_l) + V_D(D - d_l).$$

Which, reduced, becomes,

$$V_m = \tfrac{2}{3}V_{d_l} + \tfrac{1}{3}V_D + \tfrac{1}{3}\frac{d_l}{D}(V_0 - V_D).$$

Take now the general equation for the vertical plane, heretofore given,

$$V = V_{d_l} - (bv)^{\frac{1}{2}} d_{ll}^2 = V_{d_l} - (bv)^{\frac{1}{2}}\left(\frac{d - d_l}{D}\right)^2$$

and substitute in it the value of d at the surface, =0, and at the bottom, =D, and we have the two expressions following:—

$$V_0 = V_{d_l} - (bv)^{\frac{1}{2}}\left(\frac{d_l}{D}\right)^2. \quad V_D = V_{d_l} - (bv)^{\frac{1}{2}}\left(\frac{D - d_l}{D}\right)^2.$$

These values of V_0 and V_D being introduced into the expression for the value of V_m, we shall have, after reduction and transposition,

$$V_{d_l} = V_m + (bv)^{\frac{1}{2}}\left(\tfrac{1}{3} + \frac{d_l(d_l - D)}{D^2}\right).$$

And substituting this value of V_{d_l} in the foregoing general equation for velocity in the vertical plane, there will result after reduction,

$$V = V_m + (bv)^{\frac{1}{2}}\left(\frac{D^2 - 3d_l D - 3d^2 + 6dd_l}{3D^2}\right).$$

Divide the identical equation, $V_m = V_m$, member for member, by this expression, and we obtain finally the general ratio of the velocity at any depth to the mean velocity in the vertical plane, viz:

$$\frac{V_m}{V} = \frac{V_m}{V_m + (bv)^{\frac{1}{2}}\left(\frac{D^2 - 3d_l D - 3d^2 + 6dd_l}{3D^2}\right)},$$

in which d_l must be replaced by its value as deduced from the investigation of the effect of wind-force, in order that all the variables may explicitly appear.

If any value could be assigned to d in this expression, which should reduce the fraction in brackets to zero, it would follow that the depth m (which would then be d) is independent of the mean velocity of the river, v. This cannot be done; but if d be made $=\frac{1}{2}$D, the ratio is greatly simplified, and the equation becomes

$$\frac{V_m}{V_{\frac{1}{2}D}} = \frac{V_m}{V_m + \frac{1}{12}(bv)^{\frac{1}{2}}}.$$

Since both D and d, disappear from this expression, the ratio of the mid-depth velocity is independent both of the depth of the river and of the force of the wind. If the velocities in the mean of all vertical planes parallel to the current be represented by U instead of V, we shall have

$$\frac{U_m}{U_m + \frac{1}{12}(bv)^{\frac{1}{2}}},$$

in which, in a channel of rectangular cross-section, U_m would be equal to v. In point of fact, for the Mississippi, it is equal to $\cdot 93v$, the coëfficient remaining sensibly constant. Substituting this value, the authors of the report have tested the formula, by computing the ratio,

$$\frac{\cdot 93v}{\cdot 93v + \frac{1}{12}(bv)^{\frac{1}{2}}},$$

for every even foot of velocity from 1 to 8, and employing the results in the computation of mean velocities from many mid-depth velocities actually observed. The observations include a number on the Mississippi and the outlet bayous, in different stages of the water, and also those made, as before mentioned, on the feeder of the Chesapeake and Ohio canal, together with others by Messrs. Hennocque and Defontaine on the Rhine, and finally those by Mr. Boileau on his experimental wooden trough. The position of the axis, among these data, varied from the surface to a point below mid-depth, and the mean velocities varied from a foot and a half to more than four feet. The differences between the observed and computed values of $\frac{V_m}{V_{\frac{1}{2}D}}$, however, were, for the most part, practically insensible, and in the few cases in which this was not true they amounted to but two or three per cent.

The near approach to constancy, and equality of the ratio between the mid-depth velocity and the mean velocity, is easily intelligible when the fact is once detected. The resistances to motion proceed from the perimeter; that is, from the bottom and the surface. The discharge remaining sensibly constant, and, in a uniform channel, the cross-section also, whatever retards the movement at one surface necessitates an acceleration at the other.

An up-stream wind, by diminishing the velocity at the surface, creates a slight increase of slope; and this would increase the mean velocity of the stream, but for the fact that, as much as the new slope would add, the wind destroys, so that the mean velocity remains constant. What the wind destroys at the surface must therefore be compensated at the bottom; and, as these effects are propagated through the mass according to the same laws as the ordinary resistances, the curve of velocities continues to be parabolic, but the level of its axis is changed.

The simplicity of this ratio suggests an improved method of gauging streams; but into these practical details it is not important that we should enter here.

We may here conveniently present, in a single group, the most important of the formulæ which result from the investigations of which we have been endeavoring to give an account. Those which have not been fully explained, are easily deducible from those which have.

$$V=V_{d'}-(bv)^{\frac{1}{2}}\left(\frac{d-d'}{D}\right)^2. \quad V_0=V_{d'}-(bv)^{\frac{1}{2}}\left(\frac{d'}{D}\right)^2,$$

$$V_D=V_{d'}-(bv)^{\frac{1}{2}}\left(1-\frac{d'}{D}\right)^2. \quad V_{\frac{1}{2}D}=V_m+\frac{1}{12}(bv)^{\frac{1}{2}}.$$

$$V_m=\frac{2}{3}V_{d'}+\frac{1}{3}V_D+\frac{1d'}{3D}(V_0-V_D).$$

$$V_{d'}=V_m+(bv)^{\frac{1}{2}}\left(\frac{1}{3}+\frac{d'(d'-D)}{D^2}\right).$$

$$V=V_m+(bv)^{\frac{1}{2}}\left(\frac{D(\frac{1}{3}D-d')+d(2d'-d)}{D^2}\right).$$

$$d'=(0{\cdot}317+0{\cdot}06f)r. \quad U_m=0{\cdot}93v.$$

$$U_0=0{\cdot}93v+(0{\cdot}016-0{\cdot}06f)(bv)^{\frac{1}{2}}.$$

$$U_r=0{\cdot}93v+(0{\cdot}06f-0{\cdot}350)(bv)^{\frac{1}{2}}.$$

$$U_{d'}=0{\cdot}93v+\left((0{\cdot}317+0{\cdot}06f)^2-0{\cdot}06f+0{\cdot}016\right)(bv)^{\frac{1}{2}}.$$

$$U=0{\cdot}93v+\left(\frac{dr(0{\cdot}634+0{\cdot}12f)-d^2}{r^2}-0{\cdot}06f+0{\cdot}016\right)(bv)^{\frac{1}{2}}.$$

$$v=\left((1{\cdot}08U_{\frac{1}{2}r}+0{\cdot}002b)^{\frac{1}{2}}-0{\cdot}045b^{\frac{1}{2}}\right)^2.$$

(To be continued.)

ART. IV.—*Observations on some of the Double Stars;* by MARIA MITCHELL.

THE instrument with which these observations were made is a five-inch equatorial telescope, by Alvan Clark.

I have not attempted to measure the double stars of a distance less than 2″, even where I considered the telescope capable of the work, supposing that I should better meet their difficulties after longer practice in micrometrical measurements. Previous to October, 1861, the observations were made at Nantucket, in long. $4^h\ 40^m\ 25^s$, and lat. 41° 1′; since that time they have been made at Lynn, and I have called the longitude of my observatory $4^h\ 43^m\ 44^s$, the latitude 42° 28′. The measurements have been made near meridian passage, except in the case of the few northern stars. In determining the angles of position, the stars have been brought between the parallel threads of the micrometer. The powers used have varied from 75 to 200. Other things being the same, the later measurements are the more reliable, the skies of Lynn being much clearer than those of Nantucket.

I have taken great pains to notice the colors of the stars before my eye was fatigued, and have frequently noticed comparative colors. The terms yellow, pale yellow, ruddy, &c., are very vague; Sirius, α Geminorum and Capella are called white by observers, but they are decidedly unlike in color. If a color-scale, made from certain stars, could be adopted, to which other stars could be referred, the errors of eyes and observers would be eliminated; but an analysis of the ray from each star can alone decide the question of real likeness.

Name of Star.	Date.	Angle of Position.	No. of Obs.	Distance.	No. of Obs.	Remarks on color, &c.
35 Piscium,	1860, Jan. 2,	152°·2		11″·9	2	The color of the small star is peculiar; there is a brown mingling with its reddish light. The large star is light yellow. The two resemble those of γ Arietis. Air very good.
" "	1860, Jan. 3,	154 ·4	2			
	1862, Nov. 23,	149 ·4				Small star reddish-brown. Air not clear.
	1862, Nov. 30,	150 ·1		11 ·9	3	
38 Piscium,	1860, Jan. 9,	243 ·5	4			The stars differ some in color. The large one is yellow, the small one has a crimson hue mingled with the yellow. The night is good.
" "	1862, Nov. 23,	237 ·9	6	3 ·4		These observations are marked "good."
	" Dec. 4,			3 ·3	6	
35 Cassiopiæ,	1860, Nov. 8,	354 ·3	3	59 ·6	3	The colors are yellow and purplish-red.
	" Nov. 16,			59 ·6	5	The small star is of a very pretty dark red color.

Table continued.

Name of star.	Date.	Angle of Position.	No. of Obs	Distance.	No. of Obs.	Remarks on color, &c.
179 P. I. Arietis,	1861, Jan. 22,	171°·1	2			Large star pale yellow, small one lilac. The stars do not differ much is size, but much in brilliancy.
98 P. III. Eridani,	1861, Jan. 27,	230 ·6	3	6″·3	2	The night poor. The large star is orange-yellow, the small one gray.
" "	1861, Jan. 30,	237 ·7	8			The air good, but the wind high. The large star is orange, strikingly so, when compared with the star 12′ south of it.
" "	1861, Jan. 31,	238 ·7	3			The night is good, and the measurements are considered good.
32 Eridani,	1861, Jan. 31,	347 ·2	4	5 ·6	2	The small star is pale blue, the larger orange-yellow.
" "	1862, Dec. 28,	350 ·3	4			The colors are very decided, yellow and pale green.
" "	1863, Jan. 1,	350 ·8	5			Colors yellow and green.
" "	1863, Jan. 2,	347 ·2				Air poor; the stars ran together.
" "	1863, Jan. 3,			6 ·3	7	
Σ 479,	1861, Feb. 4,	130 ·2	4	7 ·2	2	Night good. The smaller star is a little ruddier than the larger. The small star mentioned by Admiral Smyth is easily seen.
Σ 494,	1863, Jan. 17,	186 ·3	4			Stars alike yellow.
Σ 546,	1861, Feb. 17,	188 ·6	3	6 ·8	3	
Σ 559,	1861, Feb. 16,	93 ·1	3	3 ·8	3	Stars much alike, color pale yellow.
"	1861, Feb. 17,	97 ·1				The night is good, and the measurements are better than those of Feb 16.
Σ 653,	1861, Feb. 21,	227 ·5	4	18 ·1	2	A faint star, seen only by glimpses, precedes.
278 P. IV. Orionis,	1861, Feb. 6,	48 ·1	4	12 ·0	3	Colors pale yellow and ruddy lilac. Admiral Smyth gives the difference in size only half a grade, in 1833; they certainly differ more at present.
118 Tauri,	1860, Feb. 9,	195 ·9	2	5 ·6	2	The large star is yellow, the small one bluish. Night good.
" "	1860, Feb. 27,	195 ·0	2			Night poor. The large star pale yellow, the small one reddish.
A Orionis,	1860, Feb. 20,	43 ·2	3			Colors yellow and bluish.
23 Orionis,	1860, Mar. 6,	30 ·4	2			The small star is of a darkish color.
		A. B.				
δ Orionis,	1860, Mar. 11,	88 ·4	2			A pale yellow, B bluish, C yellow.
		D. F.				
		22 ·7	2			
Castor,	1863, Mar. 20,	241 ·0	3			The North and preceding star is ruddier and decidedly smaller than the other.
"	1863, Mar. 21,	242 ·7	4	5 ·6	2	I call the colors warm yellow and pale yellow.
Σ 1282,	1863, Mar. 26,	257 ·9	5	3 ·3	4	A fine pair. The colors are pale yellow; the smaller has the more color.
"	1863, Mar. 27,	257 ·6	5			Small star ruddier in color than the large one.
"	1863, Mar. 30,	259 ·7	3			Air poor.
τ′ Hydræ,	1863, Mar. 29,	2 ·4	3			High wind.
	1863, Mar. 30,			66 ·8	5	The stars are much winged.
Σ 1273,	1863, Apr. 1,	220 ·5	3			The star was difficult and the night very poor.
Σ 1311,	1863, Apr. 21,	201 ·9	3			

Table continued.

Name of star.	Date.	Angle of Position.	No. of Obs.	Distance	No. of Obs.	Remarks on color, &c.
35 Sextantis,	1863, Apr. 27,	238° 9	3	6″·8	3	A blue color has been noticed in the small star, which I do not see.
	1863, May 1,	239 ·9	4	7 ·6	6	The large star is orange in color, the small star is pale, it may be called lilac.
ζ Ursæ Maj.,	1860, Apr. 30,	103 ·5	3			The stars are both yellow, but the small one has the deeper hue.
δ Corvi,	1862, May 25,	212 ·6	4			Air not very good.
	1862, May 26,	214 ·1	4			By daylight. The measurements are considered very good.
γ Virginis,	1861, June 14,	171 ·9	4			The air is tremulous and the stars run together. Both yellow. Observations marked "good."
	1861, June 18,	167 ·6	3			
	1862, June 17,	170 ·1	3			By daylight. The wind is high and the stars run together a good deal.
ξ Bootis,	1862, July 5,	320 ·6	3			The large star is pale yellow, the small one orange.
	1862, July 11,	324 ·3	4			By daylight. The measurements are not very good.
28 Aquilæ,	1859, Sept. 1,	175·17	2			Stars yellow and lilac.
" "	1859, Sept. 6,			52.12	4	
γ Delphini,	1860, Sept. 1,	273 ·9	2			Stars yellow and bluish.
Σ 2523,	1860, Sept. 18,	149 ·4	5	6 ·3	4	A beautiful pair, the stars very much alike and whiter than the stars commonly called "white." I notice a little blue tinge.
"	1860, Sept. 22,	151 ·6	2			
β Cygni,	1859, Sept. 7,	56 ·1	2			The colors are beautifully contrasted; orange and light blue.
ζ Sagittæ,	1859, Sept. 9,	313 ·6	2			
	1859, Sept 29,	314 ·1	2			
	1860, Aug 29,	312 ·9	5	6 ·9	3	The colors are pale yellow and very faint lilac.
57 Aquilæ,	1859, Sept. 30,	174 ·9	3	38 ·8		Stars yellow with a greenish tinge; the smaller star has the more green.
" "	1860, Aug. 26,	171 ·6	4	37 ·2	4	
Σ 2613,	1860, Sept. 21,			4 ·6	2	
"	1860, Sept. 22,	349 ·9	4	5 ·6	3	The stars are much alike, but the smaller is the lighter in color.
"	1860, Sept. 26,	349 ·7	5	4 ·3	3	
"	1860, Sept. 30,					I noticed the colors only. They are much alike, but the northern one is ruddier.
Σ 2621,	1860, Sept. 26,	223 ·6	3			The stars are smaller than those of Σ 2613, and there is more difference in relative size. The smaller star has a ruddy tint.
	1860, Sept. 30,					The small star seems to be of a bluish white color.
226 P. XX. Antinoi.	1860, Sept. 3,	211 ·0	6	4 ·6	4	Very good night. Colors yellow and blue. Stars very nearly of a size.
	1860, Sept. 10,	208 ·9	4	3 ·5	3	
ζ Aquarii,	1859, Nov. 23,	342 ·6	5			
"	1859, Dec. 12,	341 ·6	1			
	1859, Dec. 15,	343 ·2	2			
	1860, Oct. 27,	342 ·6	4			Stars much alike in size. Northern one smaller and yellower in color.
	1860, Nov. 1,	339 ·9	5	2 ·2	3	Some fog. Stars blurry.
	1862, Oct. 18,	339 ·4	4			Night poor. The stars run together.
	1862, Oct. 20,	341 ·9	4			Colors alike pale yellow.
	1862, Nov. 14,	341 ·1	4			

ART. V.—*On the Flora of the Devonian Period in Northeastern America;* by J. W. DAWSON, LL.D., F.R.S., Principal of McGill University, Montreal.[1]

[CONCLUDED FROM VOL. XXXV, P. 319.]

IN the course of the preceding pages, I have endeavored to notice points of general geological and botanical interest as they occurred; and it will now be necessary only to mention a few leading results, as to the Devonian Flora, which may be deduced from the observations above recorded.

1. In its general character, the Devonian Flora resembles that of the Carboniferous Period, in the prevalence of Gymnosperms and Cryptogams; and, with few exceptions, the generic types of the two periods are the same. Of thirty-two genera to which the species described in this paper belong, only six can be regarded as peculiar to the Devonian Period. Some genera are, however, relatively much better represented in the Devonian than in the Carboniferous deposits, and several Carboniferous genera are wanting in the Devonian.

2. Some species which appear early in the Devonian Period continue to its close without entering the Carboniferous; and the great majority of the species, even of the Upper Devonian, do not reappear in the Carboniferous Period; but a few species extend from the Upper Devonian into the Lower Carboniferous, and thus establish a real passage from the earlier to the later Flora. The connexion thus established between the Upper Devonian and the Lower Carboniferous is much less intimate than that which subsists between the latter and the true Coal-measures. Another way of stating this is, that there is a constant gain in number of genera and species from the Lower to the Upper Devonian, but that at the close of the Devonian many species and some genera disappear. In the Lower Carboniferous the Flora is again poor, though retaining some of the Devonian species; and it goes on increasing up to the period of the Middle Coal-measures, and this by the addition of species quite distinct from those of the Devonian Period.

3. A large part of the difference between the Devonian and Carboniferous Floras is probably related to different geographical conditions. The wide swampy flats of the Coal Period do not seem to have existed in the Devonian era. The land was probably less extensive and more of an upland character. On the other hand, moreover, it is to be observed that, when in the Middle Devonian we find beds similar to the underclays of the Coal-measures, they are filled, not with *Stigmaria*, but with rhi-

[1] Copied from the *Quarterly Journal of the Geological Society*, Nov., 1862.

zomes of *Psilophyton;* and it is only in the Upper Devonian that we find such stations occupied, as in the Coal-measures, by *Sigillaria* and *Calamites.*

4. Though the area to which this paper relates is probably equal to any other in the world in the richness of its Devonian Flora, still it is apparent that the conditions were less favorable to the preservation of plants than those of the Coal Period. The facts that so large a proportion of the plants occur in marine beds, and that so many stipes of Ferns occur in deposits that have afforded no perfect fronds, show that our knowledge of the Devonian Flora is relatively far less complete than our knowledge of that of the Coal-formation.

5. The Devonian Flora was not of lower grade than that of the Coal Period. On the contrary, in the little that we know of it, we find more points of resemblance to the Floras of the Mesozoic Period, and of modern tropical and austral islands, than in that of the true Coal-formation. We may infer from this, in connexion with the preceding general statement, that, in the progress of discovery, very large and interesting additions will be made to our knowledge of this Flora, and that we may possibly also learn something of a land Fauna contemporaneous with it.

6. The *facies* of the Devonian Flora in America is very similar to that of the same period in Europe, yet the number of identical species does not seem to be so great as in the coal-fields of the two continents. This may be connected with the different geographical conditions in these two periods; but the facts are not yet sufficiently numerous to prove this.

7. The above general conclusions are not materially different from those arrived at by Gœppert, Unger, and Bronn, from a consideration of the Devonian Flora of Europe.

ART. VI.—*Action of Bromine and of Bromhydric Acid on the Acetate of Ethyl;* by J. M. CRAFTS.[1]

MR. WURTZ proposed to me to seek to obtain from the action of bromine on the acetate of ethyl a product of substitution represented by the formula[2] $C_4H_7BrO_2$, in order to study its reaction with the oxyd of silver in the presence of water. The treatment with oxyd of silver and water, of products of substitution of bromine and iodine in organic radicals, serves to replace these elements, and consequently the equivalent of hydrogen, whose place they occupy, by the peroxyd of hydrogen,

[1] Communicated to this Journal by the Author.
[2] C=12, H=1, O=16. Doubled atomic weights of C and O.

HO, and is a reaction which of late has attracted the attention of chemists.

In the case of the body, $C_4H_7BrO_2$, which I hoped to obtain from the acetate of ethyl, it was a question of some interest to determine whether this reaction would be represented by the first or the second of the two following equations:

Glycol. Acetate of Silver.

I. $\begin{matrix}C_2H_3O\\C_2H_4Br\end{matrix}O+Ag_2O+H_2O=\begin{matrix}C_2H_4\\H_2\end{matrix}O_2+\begin{matrix}C_2H_3O\\Ag\end{matrix}O+AgBr;$

Glycocollate of Silver. Alcohol.

II. $\begin{matrix}C_2H_2BrO\\C_2H_5\end{matrix}O+Ag_2O+H_2O=\begin{matrix}C_2H_2O\\HAg\end{matrix}O_2+\begin{matrix}C_2H_5\\H\end{matrix}O+AgBr:$

in other words, whether the bromine had been substituted for hydrogen in the radical ethyl or acetyl.

As chlorine attacks the acetate of ethyl with great energy, and gives a series of products of substitution, commencing, however, only with the one containing two equivalents of chlorine, $C_4H_6Cl_2O_2$, it seemed probable that bromine would act in a similar manner, but with less violence, and that the first product of substitution, the body $C_4H_7BrO_2$, might be obtained if two equivalents of bromine for one molecule of acetate of ethyl were employed. The reaction would be represented by the equation:

Acetete of ethyl.

$$C_4H_8O_2+2Br=C_4H_7BrO_2+HBr.$$

The reaction of bromine proved, however, to be entirely different from that of chlorine on the acetate of ethyl. No product of substitution was formed, and therefore the question proposed above could not be resolved; but the results which I obtained in making the experiment possess sufficient interest to induce me to publish them.

Bromine dissolves in the acetate of ethyl with disengagement of heat, and a diminution of the original volume takes place after the mixture of two liquids has become cool; but no bromhydric acid is disengaged, and the mixture, even after it has been exposed to the diffuse light in the laboratory for several weeks, when distilled, passes over mostly between 60° and 80° C., and can be separated, by washing with a dilute alkaline solution, into bromine and acetate of ethyl unchanged.

If, however, one molecule of acetate of ethyl and two equivalents of bromine are heated together in a glass tube sealed by drawing the end to a point and melting it with the blast lamp, the color of the bromine disappears almost immediately at a temperature of 150°, or after twelve to twenty hours at 106°. No bromhydric acid is disengaged on opening the tube, and the liquid contains only a small quantity in solution, which is given

off in heating it. The liquid obtained from several operations in sealed tubes was distilled. About one-half distilled at a temperature near 45° C.; the mercury then mounted rapidly to 200°, while only a small quantity of an acid liquid passed over; the residue was allowed to cool in the retort.

The first portion, distilling near 45°, washed with a solution of potash and dried over solid hydrate of potash distilled at 38°·5–39°, and had all the properties of bromid of ethyl. An analysis gave:—

	Found.	Theory, C_2H_5Br.
C =	22·66	22·02
H =	4·55	4·59

The portion that distilled at 45° to 200° consisted mostly of acetic and bromacetic acids: when diluted with water it deposited only a small quantity of bromid of ethyl, together with a few drops of another bromated compound, whose point of ebullition was higher, but of which not enough could be obtained to enable me to determine its nature.

The liquid, boiling above 200°, which had been left in the retort, on cooling was transformed in greater part into a crystallized solid. The crystals, pressed between folds of filter-paper, and then heated to 180° in current of carbonic acid to free them from bromhydric acid, possessed the properties of bromacetic acid, $C_2H_3BrO_2$. Their solidifying point was about 46°. A determination of bromine gave:—

	Found.	Theory.
Br =	57·60	57·55

The portion of the product, boiling above 200°, which did not solidify in the retort, was without doubt a mixture of bromacetic and bibromacetic acids; it contained 67 p. c. of bromine, while bibromacetic acid contains 73·4. The quantity of this latter acid produced in the reaction was small.

It will be seen from these data that the acetate of ethyl, instead of giving a product of substitution with bromine, is decomposed by it into bromid of ethyl and bromacetic acid. The chief reaction is represented by the equation:—

Acetate of Ethyl.		Bromacetic acid.		Bromid of Ethyl.
C_2H_3O O C_2H_5	$+2Br$ =	C_2H_3O O Br	+	C_2H_5Br

The bibromacetic acid is evidently a secondary product, arising from the action of bromine on the bromacetic acid, and it only remains to account for the formation of the small quantity of acetic acid which was also observed. Thinking that the formation of one might have been dependant upon that of the other, that is, that the bromhydric acid set free by the action of bromine on the bromacetic acid might have reacted upon a portion

of the acetate of ethyl to produce bromid of ethyl and acetic acid:

$$\underset{\text{Acetate of ethyl.}}{\left(\begin{matrix}C_2H_3O\\C_2H_5\end{matrix}O\right.} + HBr = \underset{\text{Acetic acid.}}{\begin{matrix}C_2H_3O\\H\end{matrix}O} + \underset{\text{Bromid of ethyl.}}{\left.C_2H_5Br\right)}$$

I was induced to try by direct experiment whether this latter reaction really takes place.

Acetate of ethyl absorbs, at the ordinary temperature, about one and a half times its weight of dry bromhydric acid gas, but disengages it again almost entirely at a temperature near its point of ebullition. If, however, the saturated solution is heated to 100° for half an hour in a sealed tube, complete decomposition is effected; the contents of the tube separate into two layers, the upper consisting of bromid of ethyl, and the lower of acetic acid, containing enough bromhydric acid in solution to make its density greater than that of the bromid of ethyl. Thus the acetate is actually decomposed by bromhydric acid in the manner suggested above.

There is also another reaction which might have played a part in the one first studied, and have contributed to the formation of acetic acid, namely, that between bromacetic acid and the acetate of ethyl. Perkin and Duppa[3] state that bromacetic acid displaces acetic acid from its combinations with bases; an analogous reaction might also take place between the former and the combinations of acetic acid with alcohol radicals; but the absence of bromacetate of ethyl from among my products, as well as the following experiment, which shows that this double decomposition is only partially effected at a higher temperature than that employed in the reaction of bromine upon the acetate of ethyl, prove that the reaction in question did *not* play any part in the formation of the acetic acid observed above.

Experiment.—One equivalent of bromacetic acid in crystals, together with an equivalent of acetate of ethyl, was heated in a sealed tube to 180° during three hours; at the end of this time the greater part of the crystals had disappeared, and, on distilling the contents of the tube, besides acetate of ethyl and bromacetic acid, a considerable quantity of an acid liquid distilling between 100° and 170° was obtained. This liquid was diluted with a large quantity of water, and the portion which did not dissolve was washed with a very dilute solution of caustic potash, and dried over chlorid of calcium; it began to distil at 80°, at which point a little acetate of ethyl passed over, the temperature then rose rapidly, and remained constant between the limits, 156° to 158°, while the greater part of the liquid distilled. An analysis of this latter portion gave:—

	Found.	Theory, $C_4H_7BrO_2$.
C =	29·35	28·74
H =	4·46	4·19

[3] *Quart. Journ. Chem. Soc.*, xi, 22.

The boiling point of bromacetate of ethyl is 159°, and its properties correspond with those of the liquid obtained above; the slight difference in boiling point and composition arises without doubt from traces of acetate which the liquid still contained.

The portion which dissolved in water was a strong acid; the solution, neutralized with carbonate of lead and evaporated over sulphuric acid, gave large and well formed crystals, whose form and angles, measured with a hand goniometer, corresponded to those described by Gerhardt as belonging to acetate of lead. The double decomposition which takes place here is represented by the equation:—

Acetate of ethyl. Bromacetic acid. Bromacetate of ethyl. Acetic acid.

$$\begin{matrix} C_2H_3O \\ C_2H_5 \end{matrix}O + \begin{matrix} C_2H_2BrO \\ H \end{matrix}O = \begin{matrix} C_2H_2BrO \\ C_2H_5 \end{matrix}O + \begin{matrix} C_2H_3O \\ H \end{matrix}O.$$

The decomposition of acetate of ethyl by bromine is analogous to that which Mr. Gal[1] has observed with anhydrous acetic acid and chlorine:—

Anhydrous acetic acid. Chloracetic acid. Chlorid of acetyl.

$$\begin{matrix} C_2H_3O \\ C_2H_3O \end{matrix}O + 2Cl = \begin{matrix} C_2H_3O \\ Cl \end{matrix}O + C_2H_3OCl$$

and this reaction is another among the many which tend to demonstrate the close analogy existing between the composition of the ethers and that of the anhydrous acids.

Paris, March 3, 1863.

ART. VII.—*New Facts and Conclusions respecting the Fossil Footmarks of the Connecticut Valley;* by EDWARD HITCHCOCK.

I HAVE devoted a considerable time during a few years past to the preparation of a Descriptive Catalogue of the Fossil Footmarks in the large collection of Amherst College. Moreover, during the past winter (1862–3) I have made a large addition to the Cabinet by the purchase of Mr. Roswell Field's private collection. Unexpectedly, many new facts have been brought to light, not contained in my "*Ichnology of New England*," published by the Government of Massachusetts in 1858, which have an important bearing on the fundamental principles of this science. Though I have been led to give up ten or a dozen of my old species of footmarks, I have described over 30 new ones, in a paper lately read by me before the American Academy of Arts and Sciences in Boston. But it is the results as to the principles of the science contained in that paper, which seem to me of most importance, and I venture, without asking leave of the Academy, to send you a few pages of the latter part of the paper for this Journal. The paper is largely illustrated by drawings

[1] *Comptes Rendus*, liv, 570.

and photographs, and, should the Society publish it, these I think will fully sustain the conclusions at which I have arrived.

The collection of Footmarks at the College, whose examination has led to the following unexpected conclusions, is now quite large. I have counted the number of individual tracks and found them to be over 20,000; but several thousands of them are the tracks of insects and small crustaceans. It was, however, some of the specimens in the collection purchased the past winter, by the generous contributions of the friends of science, that first opened my eyes to the facts detailed below.

Supposed Mistake as to the number of Phalanges in some of the Lithichnozoa.—It is well known that the number of phalanges and their order, in the toes of living birds, enable the anatomist to distinguish them from other animals, with only a few exceptional cases. In four-toed birds it is two in the inner toe, three in the second, four in the third, and five in the outer toe, and where there are only three toes, the numbers are the same as in the three outer toes of the four-toed birds. But since the penultimate and ungual phalanges would make only one impression, we should expect in the track that the numbers would be one less than above indicated. And such they seemed to be to every observer without exception in the three-toed pachydactylous Lithichnozoa, viz: two in the inner toe, three in the middle and four in the outer toe. This of course was regarded as the grand argument to prove them made by birds.

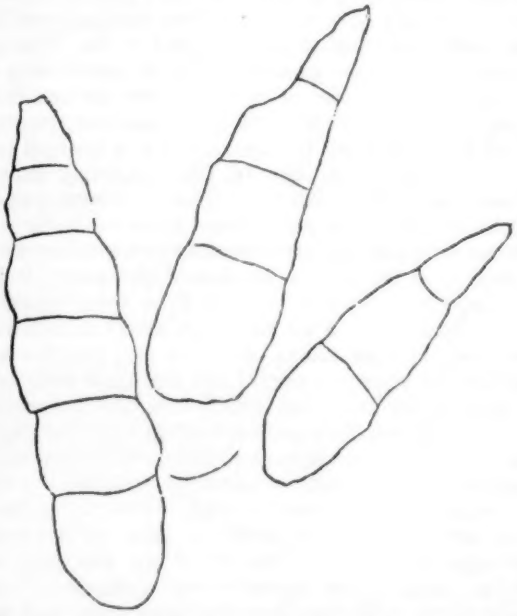
For some time past my suspicions have been that we have all been mistaken as to the true number of phalanges, and when I went into an examination I found it even so in respect to the outer toe. By looking at the drawings which myself and others have published of these tracks, it will be seen that what we have supposed the posterior phalanx, in that toe, lies wholly behind the first phalanx of the inner and middle toes, and sometimes also a little out of the line of the other parts of the toe. Now by looking at the feet of the different species of birds, either in a cabinet or in drawings, we shall see that the posterior phalanges in the three toes lie nearly abreast of one another, unless it be the middle toe where this phalanx is usually a little in advance. This posterior impression, behind the outer toe, was not, therefore, made by a phalanx, but probably by a process of the tarso-metatarsal bone. We accordingly sometimes find a similar posterior impression behind the inner toe, and indeed a thin and smaller imprint of this sort shows itself sometimes, as on the sketch on page 782 of my Ichnology, and on figs. 21 and 22 of the present paper.

This fact, I confess, very much unsettled my conviction that any of the Lithichnozoa were birds. And they were still farther shaken by the facts I have already detailed respecting that most

anomalous animal, the Anomœpus. The trifid tracks of its hind feet had been mistaken by us all for those of birds. Indeed, the number of phalanges in the toes were found without much doubt to correspond with those of living birds, and also with those Lithichnozoa which I had regarded as birds. But the Anomœpus had been proved without question to be four-footed: Are we not forced to the conclusion that all the Lithichnozoa with similar trifid feet must be quadrupeds?

Another development as to the Phalanges.—Probably I should ere long have come to this conclusion, had not another discovery awaited me. Among the new specimens purchased was one very beautiful row of thick-toed trifid tracks, such as we had been in the habit of supposing made by birds; but I have little doubt that they were those of an Anomœpus, though no marks of fore

1.



feet or tail are seen. I have named it as a new species (the *A. curvatus*) of that genus, though differing but little from the *A. intermedius* already described. On looking at these tracks I was surprised to find in the outer toe, *four very distinct phalangeal impressions besides the posterior imprint*, which I now look upon as made by a heel bone not a phalanx. The sketch annexed gives an exact outline of one of these tracks, and on examining

the remarkable slab of *Anomœpus intermedius* already described in this paper, I found on that, also, evidence that in some cases the outer toe had four phalangeal impressions beside the heel bone. So far as the Anomœpus is concerned, then, I feel sure that we have in its phalangeal impressions the normal number and order in the feet of living birds. I was at once led to inquire whether the same thing might not be true in respect to those thick toed Lithichnozoa which I have regarded as birds. I have found proof enough to satisfy myself that it is so, and that the reason the fact has been overlooked is that the penultimate and ultimate phalanges (omitting the ungual) rarely made separate impressions. But occasionally I can see a faint line of demarcation between them. But I had frequently noticed that the length of the ultimate phalangeal impression on the outer toe (as a reference to the outlines of these tracks in the *Ichnology* will show) was as long as, and sometimes longer than, those which preceded it, whereas, so far as I have examined the osteology of birds' feet, they decrease in length toward the extremity. I think that generally two phalanges have been mistaken for one, in this part of the toe.

If these are probable conclusions they lead to important results. The first is, that if we strike off the posterior impression of the outer toe in the thick-toed bird tracks referred to, we shall still have the normal number of phalanges in the feet of living birds. But the same thing is proved still more decidedly in regard to the Anomœpus, which is four-footed. Hence the conclusion follows, that in the fossil footmarks birds cannot be distinguished from quadrupeds by the number of phalanges. This law of correlation among living animals would seem not to have been true with the fossil.

How far do the Protuberances on the feet of animals correspond with the Phalanges?—This subject could not but engage my attention in the progress of these investigations. But not finding it discussed by any anatomical author, and being prevented by feeble health and winter weather from access to any large collection of animals, I have been able to arrive at only unsatisfactory results. My examinations have been confined chiefly to the feet of birds, and the following facts have been obtained. The most important question under consideration is this:—Is it the phalanges or the articulations of the toes that make the deepest impression on mud or other plastic material trod upon? This will be determined by finding under which of these parts the protuberances are the most prominent. If under the phalanges, the number in the toe will be one less than if under the articulations; that is, if we count, as one of them, the articulation with the tarsal or tarso-metatarsal bone.

The protuberances on the foot of the turkey, both wild and tame, correspond neither with the phalanges nor the articulations, but are more numerous than either. The same is true of the domestic hen. There is a general resemblance, however, in this respect, between different individuals of these genera.

In the *Botanus lentiginosus*, the protuberances seem to correspond with the articulations, or joints.

In the Coot, the wings along the toes expand most in the middle of the phalanges.

In the Crow, the correspondence seems to be essentially with the articulations, judging from some tracks of this bird on clay in the cabinet. But the Struthionidæ have feet more nearly resembling the tracks under consideration. And in the *Rhea Americana* or South American Ostrich, although these protuberances are tolerably distinct on the middle toe, yet the inner and outer toes do not show them. A large heel shows itself behind the middle toe.

(Casts of the feet of the above bird were exhibited to the Academy).

These few examples show that there is a great diversity among living birds in the matter under consideration. Sometimes the protuberances correspond with the articulations, sometimes to the phalanges, and sometimes to neither. But I have never found feet that would make such distinct and marked tracks, and with always the same number of rounded impressions, as did the thick toed Lithichnozoa; and I am still inclined to believe that such was the structure of their feet that their tracks would show the number of phalanges rather than of the articulations. It could not be the latter, if the views I have presented in this paper as to the posterior imprint in the outer toe be correct; for that impression is entirely behind the phalanges on all the toes. I could wish, however, that I had time, strength, and opportunity, to pursue this subject farther among existing species of animals.

But, though my researches have been unsatisfactory on the particular point above mooted, they seem to me to have settled another of much interest. I do find protuberances on the feet of birds, especially the tridactyle species, behind the phalanges, such as might well have left those impressions on the tracks which we have mistaken for the posterior phalanx. We are thus relieved from the necessity of supposing anything peculiar in the processes of the tarso-metatarsal bone in the fossil animals.

The Feathered Fossil of Solenhofen.—The recent discovery of a remarkable animal, called by some Griphosaurus, and by others Archæopteryx, in the famous lithographic quarries of Solenhofen in Bavaria, throws some light, I think, upon the thick toed Lithichnozoa, while these reflect some light upon the feathered fossil. For it had feathers, yet some of the ablest zoologists

pronounced it a reptile. Others, however, as Prof. Owen of London and Prof. Dana of New Haven, believe it to have a predominence of ornithic characters, so as to make it a bird. Some important parts of the skeleton are wanting, as the head, neck, dorsal vertebræ, and sacrum, and the ribs are detached and scattered about. The forearm consists of radius and ulna, a metacarpal bone, and a few detached small fingers; also two small slender bones with sharp claws like those on the hind foot, which may have been used for clinging, or as weapons of offense.

The lower right limb consists of a femur, tibia, and tarso-metatarsus, to which one hind toe and three foretoes are articulated, the phalanges being one, two, three, and four, though the last number is a little doubtful, on account of the position of the outer toe. The toes are all armed with sharp claws.

The tail is six inches in length and consists of twenty vertebræ, of narrow elongated form, diminishing in size to the last. The feathers of the tail are attached in pairs to each vertebra throughout its entire length.

Now between these characters and those of some of our Lithichnozoa there are some remarkable analogies or resemblances, so far as I can judge, and which I would now indicate —at least such as have arrested my attention, with some of the inferences that follow. It is perhaps unexpected that they ally the Archæopteryx rather to the quadrupedal Anomœpus than the biped tridactyles in my *Ichnology*.

1. In both we have on the hind foot three front toes articulated to a stout tarso-metatarsal, and not as in all animals except birds, to a tarsus of several bones. This resemblance applies also to the biped, thick-toed, tridactyle Lithichnozoa, as well as to the Anomœpus, for they must all have had tarso-metatarsals below the tibia and fibula, though no impression among the tracks indicate any such bone. But we have the most decisive evidence that these animals had only three toes, and where in existing nature do we find that number articulated with anything but a tarso-metatarsal, except a few cases in the Ruminantia and Solipedia.

2. They both had the same number of phalanges in the three front toes, though a little doubt remains as to the outer toe of the latter. The same number of phalanges existed in biped Lithichnozoa so far as we can judge by their tracks.

3. The posterior extremities of both, as far as the tarsal joint, correspond exactly with those of living birds: hence the tracks of the hind feet of the Anomœpus, as well as those of the trifid Lithichnozoa under consideration, are pronounced at once, on first seeing them, to have been made by birds; and it is only when we occasionally see where the Anomœpus brought its fore-

feet to the ground, that we suspect it could have been four footed.

4. Precisely how much correspondence there may be in the anterior extremities of the two animals we cannot decide. The Archæopteryx is thought to have had but one metacarpal bone, and the fingers are so scattered that their number is not given, but two are described as slender with long claws. The most perfect track of the fore-foot of the Anomœpus has five toes, the two hindmost showing two phalanges, the third, four, the fourth, three, and the farthest two. The four last toes at least show small claws. The fingers are arranged so as to be fan-shaped, all pointing more or less outward, resembling an expanded wing. But they seem to be genuine fingers, and there is no appearance of feathers on any of the tracks, on the hind or fore-feet. The figure annexed shows an outline of the most perfect track of the fore-foot yet found.

2.

This certainly looks more like the fore-foot of a lizard, and still more like that of some mammals, than the forearm of a bird, and it is difficult to conceive how it could have been used as an organ of flight, though possibly it might have been employed for prehension. But on the other hand we have conclusive evidence that it was not used for walking, except perhaps occasionally, and imperfectly. The right and left anterior feet that made the tracks were placed almost invariably nearly abreast of each other, as if the animal were resting, and not in alternation as in walking. But of more than forty steps of *Anomœpus intermedius*, shown on the remarkable slab described in this paper, the fore-feet show themselves only twice, and that when the animal rested. Indeed, we may safely assume that the principal object of the fore-feet was not locomotion, and the same remark is applicable to other species, even the gigantic Otozoum. What other purpose in the economy of these animals could have been subserved by such a structure, except perhaps prehension, I will not attempt to decide. Yet the fact has awakened an inquiry whether such a structure may not have existed in an animal whose predominant characteristics were those of a bird.

5. But there was a tail, and how shall we reconcile that fact with an ornithic character? It might have been impossible before the discovery of the fossil at Solenhofen. But that animal had a tail six inches long with twenty vertebræ, and yet the most eminent zoologists regard it as a bird. The characters of the tail in the Anomœpus are very peculiar, yet there are some curious resemblances between its markings on stone and the tail of the Archæopteryx. The traces of the tail of the Anomœpus

have three distinct phases. The largest species left a heart-shaped indentation, which was repeated every few inches. Would not such impressions be just what we might expect if this animal had such a short blunt tail as the Archæopteryx? And does it not suggest one of the uses of such a tail, viz: to furnish the animal with a sort of third hind-foot to help sustain it while it might use its fore-feet perhaps for seizing upon objects above and around it.

The tail of the *Anomœpus intermedius*, although rarely leaving an impression, did sometimes drag along and make a narrow continuous trail. This would indicate greater length and perhaps tenuity. But how much of attenuation and elongation might be consistent with an ornithic type we have no means of knowing. Prof. Dana speaks of a posterior elongation of the body as "connected profoundly with inferiority of grade in the different types of animal life," and says, that "it is the very one of all abnormal features to be looked for in the early birds."

Upon the whole the singular markings of the tail upon stone, with the exception perhaps of *A. intermedius*, do really suggest a curious coincidence between the the caudal extremity of this genus and that of the Archæopteryx.

Just as I had reached this point in my conclusions, a curious development awaited me. In examining some new specimens, a singular trail showed itself upon one which I had never before noticed; or if I had seen it, I had not connected it with the tracks, but considered it among those inexplicable markings due perhaps to water and wind, which so frequently puzzle the student of ichnology. But in this case there is a series of some six or seven rather flat and broad grooves, each one or two tenths of an inch wide, and the whole forming a trail more than an inch wide, running across the entire specimen, passing over one very distinct three-toed narrow-toed track, which is half an inch deep, and the grooves show themselves on opposite sides of the footmark, certainly two thirds of its depth, appearing as if some flipper-like appendage had dragged behind the animal, capable of easily conforming itself to the irregularities of the surface. The fact, that the marks follow the depression of the track, shows that they were made subsequent to the track, and suggests at once the idea of a broad and singular tail. What a pity it is that there is only one track upon the specimen: but so far as I can judge, the trail runs in the direction in which the animal was moving.

In these conclusions I should have acquiesced with considerable confidence, had I not found, on examining our new specimens, as well as others in the cabinet, that we had quite a number with similar markings, and that the trails in these do not always follow the line of tracks, but are sometimes on one side of it and

sometimes on the other; now and then on both sides, and then crossing the line of tracks, so as to seem to have no connection with them. In general, however, it seems as if some flipper-like appendage to the posterior part of the animal had been thrown out on one side and the other, making sweeps occasionally, so as to leave curved trails. The species of track with which they are most usually associated is the *Anisopus gracilior*, described in the first part of this paper. In this species both hind and fore-feet are almost always shown on the stone, and it is quite obvious that the flipper-like impressions have no connection with the feet. They seem also rather large for the tail of so small an animal, whose feet are all less than an inch. I have hence been sometimes inclined to believe that the trails were made by some animals swimming along near the bottom, and occasionally striking and grooving it with its flippers or fins. But my more mature conviction is, that they are connected with the tracks. But it needs a series of expensive drawings to make the facts fully understood without specimens.

6. But to return to the Anomœpus; which characters shall we now regard as predominating in its structure and movements, those of the bird, or those of the lizard, or of the mammal? It is difficult to avoid the conclusion that the ornithic characters are the most numerous and striking. It may, after all, have been a bird, of so low a grade, however, that, even with its skeleton before him, the anatomist would hesitate where to place it, as in the case of the Archæopteryx.

7. This conclusion, to which the facts and reasoning have conducted me, not without remaining doubts, would, not long since, have appeared very absurd. But, if it could be admitted, see what a relief it would give to difficulties. If the Anomœpus were a lizard, or marsupial, we must give up that firmly established law of correlation which enables us to distinguish different classes of animals by the number and order of phalanges; but if it were a bird, that law can still be reckoned upon among the fossil as well as living animals. If a bird, we can see also how it was that it generally walked upon two feet, although it had another pair, to be used perhaps for several purposes, but rarely for locomotion.

8. If we can presume that the Anomœpus was a bird, it lends strong confirmation to another still more important conclusion, which is, that all the fourteen species of thick-toed bipeds, which I have described in the *Ichnology*, and in this paper, were birds. In this case, if we can retain the law as to the phalanges, all the characters of the animal, as made known by their tracks, belong to birds, with little variation from the existing bird type. They were bipeds unquestionably. Since they are the most abundant of the tracks, I have now seen thousands of them, and had fore feet existed I am sure they would occasionally have left some

trace of them, as is the case with every other species of Lithichnozoa. They had but three toes: at least, if a fourth existed in any case, it must have been articulated so high as not to reach the ground. These three toes are articulated to a tarso-metatarsus, as is the case with nearly all tridactyle animals. They had the same number of phalanges as the birds.

The impressions left by the cushion beneath those processes of the tarso-metatarsus which form the heel correspond to those which living birds would make, and, so far as I have examined, not to those of any other class of animals, though my examinations on this point have been few. The claws and papillæ agree essentially with those of birds. Finally, the great length of stride in some cases, and the position of the tracks nearly on a right line, indicate long legs of wading birds, and not any other kind of animal.

Most of these arguments are good for the ornithic origin of these tracks, whatever opinion we may entertain as to the Anomœpus. The only difference is, that, if we regard it as a reptile, the argument from the number of phalanges must be given up; if as a bird, that strong evidence is retained. But even without this, I cannot hesitate to reckon the biped thick-toed Lithichnozoa as birds; for I see no characters in their tracks that ally them to any other animals. I must consider them not only as birds, but as forming a quite perfect type of birds for sandstone days. The analogies taught us by palæontology (see Prof. Dana's appended letter) would lead us to expect also in the same period a lower group of birds, and these may have been the Archæopteryx and perhaps the Anomœpus, with some other genera of Lithichnozoa which I might name.

How then could I avoid the conclusion that these animals were birds? Doubtless with some peculiarities of structure, bringing them into the "*comprehensive types*" of Dana, but still decidedly birds. When I began this paper, and ascertained that we had probably made a mistake as to the number of phalanges, I felt as if this opinion, which I have always maintained, was becoming doubtful. But new examinations brought new facts to light, and the history of the Solenhofen fossil added others, until it appears to me we may now with more confidence than ever maintain the *avian*[1] character of these animals. It is certainly gratifying even to seem to touch soundings, after having been so much tossed on the sea of difficulty, and I cannot but hope that subsequent researches will show that we have not cast anchor merely in quicksand.

Prof. Dana's Views.—Having occasion while engaged in the investigations detailed in the preceding paper to write a letter on business to Prof. J. D. Dana, I mentioned some of the results to

[1] A new and much needed word used by Professor Richard Owen in a recent letter, and which I venture to introduce into print.

which my mind was coming. His reply contained too much good reasoning and important suggestions to be lost, and I venture, without his leave, but trusting he will excuse me, to annex his remarks to this paper.

"New Haven, Feb. 7th, 1863.

My Dear Sir:—Your new results from recent researches among the tracks of the Connecticut valley are of great interest, and I should be glad to put your conclusions, when you are ready with them, in the Journal. I am satisfied that we cannot infer the form and character of the earliest birds from those of the present day. The early type is evidently one of the mixed types ('comprehensive,' as I have called them) which diverged widely from the normal type—just in fact as the Ganoids diverge from the Ctenoids and Cycloids—the Marsupial from ordinary Mammals —and Amphibians (or Batrachians) from true Reptiles. You know that in the class of Mammals the Marsupials are *semi-oviparous*, or intermediate between the true viviparous mammals and the oviparous birds and reptiles. So again the Amphibians are intermediate between true reptiles and fishes, having gills when young like fishes, etc. Now by the recent discovery of the feathered fossil of Solenhofen, we have a corresponding inferior division of birds intermediate between birds and reptiles. Thus each class has its great typical group and its inferior abnormal group, related to the class next below. This being so, wide divergencies of form in the abnormal group are to be looked for.

There is another principle bearing on this subject—*the remarkable harmony among the types of any era through the past ages.* Thus the coal plants are made up mainly (1) of the highest Cryptogams, that is, the Acrogens, of which the Fern is the typical group, (2) the lowest Phænogams, the Conifers, and (3) intermediate (or comprehensive) types in each class, Lepidodendra of the Lycopodium tribe, a type coniferous in habit, and Sigillariæ of the Phænogams, also intermediate between Conifers (in the Gymnosperms) and the Lepidodendra. By such an assemblage, the flora was rendered remarkably harmonious. Had the progress of life consisted in an advance of Cryptogams to Mosses, along with the introduction of Conifers, it would have been far otherwise.

Again, (1) the Reptilian fishes (Ganoids), (2) Amphibian Reptiles, and (3) fish-like higher Reptilians (Marsh's Eosaurus) made up a harmonious assemblage of Carboniferous *animal* life. Again, the semi-oviparous Mammals (Marsupials) and oviparous Reptiles, &c., were in harmony with one another; and if true non-marsupial Insectivores appeared also, it was still in harmony; for the Marsupials were mainly Insectivores: moreover the former were prophetic of the higher development of the Mam-

malian class. Now, if, along with the semi-oviparous Mammals and swimming, crawling, and flying Reptiles, there were Reptilian Birds, waders and others, the harmony would be only the more complete. The presence of the same number of phalanges in birds and reptiles would be not at all improbable—certainly no basis for an argument against the birds."

In another business letter of Feb. 14th, I find the following:

"The strongest argument for the ornithic character of the feathered fossils are, (1) *that the animal had feathers:* for the idea that they were not true feathers is a mere supposition without any facts to sustain it: (2.) That the *expanse of the wing was made by feathers on a short arm*, and not as in the Pterodactyl by an expanse of the skin supported by an elongated finger. The structure of the foot in the Pterodactyl also shows that the animal had no close relation to the Birds. The world will have finally to settle down to the belief that there were Reptilian Birds in ancient times, as well as Ichthyoid Reptiles and Oöticoid Mammals. This is my strong persuasion."

Amherst, Mass., May, 1863.

Correction for part of edition.—On the preceding page, in the last paragraph, after (2), the word Amphibians should be inserted.

ART. VIII.—*On Hydrastine;* by F. MAHLA, Ph.D., Chicago, Ill.

HYDRASTINE was detected by Durand, in Philadelphia, as early as 1851, who noticed its alkaline nature, but did not succeed in preparing it in a pure state.

It is contained in *Hydrastis Canadensis*, in which it is associated with berberine. Mr. J. D. Perrins, of Worcester in England, first separated it from this plant in pure form, and described some of its properties, but did not institute an elementary analysis. At thet ime of Mr. Perrins' investigations, I had, prompted by the remarks of Prof. Procter of Philadelphia, also prepared pure hydrastine, and intended to analyze it; seeing, however, that Mr. Perrins had promised to study its composition, I did not continue my investigations. After the lapse of more than a year, I do not any longer hesitate to finish my researches, and take the liberty to publish herewith the results.

Hydrastine may be obtained by adding aqua-ammonia in slight excess to the liquid from which the berberine has been previously separated by an addition of chlorhydric acid. The precipitate, obtained under these circumstances, is collected on a calico filter, freed by expression from water, and mixed with strong alcohol, in which it easily dissolves by application of heat. On cooling, the hydrastine crystallizes readily, and may be pu-

rified from adhering coloring matter by redissolving and recrystallizing it several times with alcohol.

Hydrastine crystallizes in forms which belong to the right prismatic system. They are combinations of the longitudinal with the vertical prism, in which the planes of the first mentioned form are prevailing. It is perfectly white, and its crystals exhibit great brilliancy. Hydrastine in the pure state is tasteless; its salts, however, have a bitter, heating, acrid taste. It melts like a resin, when heated to 135° C.; it decomposes at a higher temperature with emission of yellowish vapors, the odor of which resembles that of carbolic acid. When heated on platinum foil, it readily takes fire and burns with a smoky flame.

Hydrastine is insoluble in water; it dissolves, however, in alcohol and in ether. It is not affected by a dilute solution of caustic potassa, even if boiled with it for a prolonged period. Concentrated nitric acid does not at first act on it, but dissolves it afterwards with a red color.

Hydrastine dissolves in cold concentrated sulphuric acid, and imparts to it a yellowish tint; this mixture when slightly warmed, assumes a red color; bichromate of potassa produces with it a dark brown coloration, which is distinct, however, from the strychnia reaction, in as far as it does not show any blue or violet shades.

It dissolves readily in diluted hydrochloric acid; ammonia and caustic potassa produce in this solution white precipitates, which are insoluble in an excess of the reagent; ferrocyanid and iodid of potassium generate also white deposits; iodine dissolved in a solution of iodid of potassium produces a cinnamon brown precipitate, which, when heated with the liquid in which it is suspended, contracts readily to a resinous mass.

Bichlorid of platinum precipitates the solution of muriate of hydrastine with a yellowish red, chromate of potassa with a yellow, color. This latter precipitate dissolves when heated with the liquid, in which it is suspended, but separates again on cooling; before dissolving, it assumes, in the liquid, the appearance of a melted resin.

Terchlorid of gold produces in the solution of muriate of hydrastine a reddish-yellow precipitate, which also contracts in the liquid on application of heat, and looks like melted resin; it however finally dissolves. Sesquichlorid of iron produces no change.

In order to subject the hydrastine to an elementary analysis, it was desiccated at a temperature of 100° C. until it ceased to diminish in weight.

The analysis itself yielded the following results:

I. 0·5013 hydrastine, burned with oxyd of copper, gave
1·2260 carbonic acid, and
0·2712 water.

II. 0·5085 hydrastine, burned with oxyd of copper, gave
1·2377 carbonic acid, and
0·2608 water.

III. 0·4469 hydrastine, burned with soda-lime for nitrogen-determination, gave
0·2727 ammonio-chlorid of platinum.

IV. 0·5904 hydrastine, burned with soda-lime, gave
0·3542 ammonio-chlorid of platinum.

These results lead to the following percentage composition:

	I.	II.	III.	IV.
C =	66·696	66·379		
H =	6·010	5·698		
N =			3·832	3·767

In order to determine the formula of the alkaloid, I selected the platinum double salt, prepared by precipitating the hydrochlorate of hydrastine with chlorid of platinum, and the hydrochlorate itself.

The chloroplatinate of hydrastine is an amorphous reddish-yellow powder, which is slightly soluble in water, better, however, in alcohol. When its alcoholic solution is boiled for some time, the platinum separates in the form of a black powder. The liquid, in which this deposit forms, has a remarkable blue fluorescence.

Chloroplatinate of hydrastine fuses, when heated to little above 100° C.; it decomposes readily when heated higher.

1·0079 chloroplatinate of hydrastine, desiccated at 100° C., gave on careful ignition
0·1630 platinum.

This corresponds to 16·17 p. c. platinum.

The chlorhydrate of hydrastine is obtained by dissolving pure hydrastine in diluted chlorhydric acid. The solution obtained is evaporated over the water-bath to dryness. It then forms a gum-like white substance, which can be readily powdered. It is easily soluble in water and alcohol. Its aqueous solution has a strong blue fluorescence. It is uncrystallizable.

In order to determine the proportion of chlorhydric acid, which is in combination with the hydrastine, I dried the powdered salt for a long time at a temperature of 100° C., and then precipitated its aqueous solution with nitrate of silver:

0·7258 hydrochlorate of hydrastine gave
0·2419 chlorid of silver.

This amount corresponds to 8·46 p. c. of chlorhydric acid.

From these results, I calculate $C_{44}H_{24}NO_{12}$ as the formula and equivalent for hydrastine. Indeed, if we figure the percentage composition of a body with this formula, and compare it with the obtained data, we find that they agree well:

Hydrastine = $C_{44}H_{24}NO_{12}$ in 100 parts:

		Found.		Calculated.
Carbon,	=	66·696	66·379	66·333
Hydrogen,	=	6·010	5·700	6·030
Nitrogen,	=	3·832	3·767	3·517
Oxygen,	=	23·462	24·154	24·118

The chloroplatinate of hydrastine, $C_{44}H_{24}NO_{12},HCl+PtCl_2$, requires in 100 parts 16·32 p. c. platinum; I found 16·17 p. c.

The chlorhydrate of hydrastine = $C_{44}H_{24}NO_{12},HCl$, contains, according to its formula, 8·34 p. c. chlorhydric acid, while I found 8·46 p. c.

Chicago, Ill., March 31, 1863.

ART. IX.—*Description of a Photometer;* by Prof. O. N. ROOD.

DOVE, a short time ago, proposed a photometer, consisting essentially of a compound microscope provided with a microcopic photograph, the latter being used as a "screen," and illuminated from both sides, in such a way that when the compensation had been reached, the photograph was rendered invisible.[1] In the construction of the instrument at present described, I have availed myself of this general mode of comparison, discarding however the microscope and photograph, and supplying their place in a more simple, and, for many purposes, in an at least equally efficient manner.

The instrument consists of a board from 50 to 100 inches in length and 12 inches wide; a slide at S supports a small lamp or standard candle; the slide can be moved towards or away from the screen P by a light rod R; the screen, reckoning from L, is composed of four parts, disposed thus: 1st, a glass plate is coated with a collodion sensitized with iodid of potassium and of the variety called adherent; this is immersed in a solution of nitrate of silver, just as though it was to be used for the reception of the camera image; it is then exposed to broad daylight for two minutes, and developed by sulphate of iron, washed and dried. By this operation a dense and absolutely opaque deposit of silver is produced in the substance of the collodion. The collodion film is then removed from one spot ½ an inch square; this can be done neatly and completely by the use of a needle, great care being taken to leave the edges of the little square sharp and clean. The observations are made by means of this spot, and it of course is to be located in the axis of the instrument. On account of the uniformity of the appearance of this plate by

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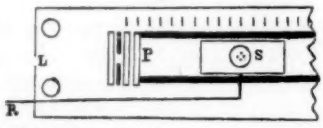


[1] *Pogg. Annalen*, Band cxiv, page 145.

reflected light, and the sudden, sharp transition from the thin opaque edges of the film to the clear glass, the screen is, I suppose, better adapted for use than any which has yet been proposed. The collodion side of the glass plate is turned towards L.

2d. Next in order comes a piece of blackened card board of the same size with plate No. 1: this is provided with an aperture corresponding with, and somewhat larger than, the exposed portion of the collodion plate.

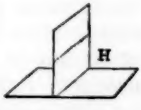
3d. Then follow two thin finely ground glass plates of the same size with No. 1, the ground surfaces facing towards L. A single plate of ground glass can be used, but with two plates the illumination of the square spot is much more uniform and even; in other words, by the use of two plates all idea of the ground glass *texture* is removed, and nothing is seen but a square patch of light.

Two small lamps or standard candles are placed about as indicated at L. The construction of the lamps is the same with those used by Potter, (*Physical Optics*, page 112,) in his photometric experiments, consisting, viz: of shallow cups filled with oil, which support little metallic bridges, formed of thin plates of metal perforated with four holes, through two of which small wicks are drawn, the others supplying air. When properly arranged these lamps will give a pretty uniform light for thirty minutes together; but though by no means furnishing a truly constant illumination, yet the variation is almost always gradual and steady, a point of great importance, for this being the case, it is easy to make the observations in such a manner as nearly to exclude errors from this source, as will presently be shown.

The movable lamp is provided with a vernier, and a scale divided into inches and tenths extends from that ground glass surface next to the collodion plate along the entire length of the board. At L is a shade to protect the eyes while observing; this has an aperture one inch square placed in the axis of the instrument. The photometer, when arranged as above described, is peculiarly adapted to measure the amount of light transmitted by plates of colorless or colored substances, as well as the amount of light reflected from polished or unpolished plane surfaces at various angles of incidence. For these uses it was expressly contrived, but at the same time it is plain that by modification it can be adapted to other purposes.

Mode of using the instrument.—The three lamps, after being lighted, are allowed to burn for twenty or thirty minutes, till their light has become steady, then the centre of the flame of the slide lamp is made to coincide with the vernier by using the arrangement seen in fig. 2; this is constructed of wood and all its angles are right angles. The vernier being placed at H, the eye is brought opposite the dotted line

2.



in such a way that the surface of the upright is reduced apparently to a line, when the lamp is moved on the slide till this line bisects the flame. The slide is then placed about as distant from the screen as may be desired and, compensation approximately effected, by moving the pair of lamps at L away from or towards the collodion surface. If these are brought too near the screen, the square spot will appear dark on a light ground; if too far off, light on a dark ground. The final compensation is made by moving the slide lamp S; this takes place when the spot becomes invisible. In the first experiments, instead of a *pair*, a single lamp was used, and owing to this the spot never became entirely invisible; one half was always faintly lighter than the adjacent ground, the other half darker; this was owing to the slight difference in the distances of the sides of the square from the lamp, and may be adduced as a proof of the delicacy of the photometer. The observer then reads off on the scale the distance of the slide lamp from the ground glass surface next to the collodion plate, the white page of the note book reflecting sufficient light for the purpose. Supposing it is desired, for example, to measure the amount of light transmitted by a plate of polished glass at a perpendicular incidence, the plate is placed at P, when it will be found necessary to bring the slide lamp nearer in order to effect compensation. The amount of light transmitted is then at once calculated by the law that "the intensities which measure the values of the illuminating powers are *directly* as the squares of the distances from the screen."

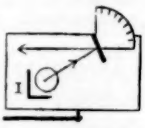
My mode of work, in all cases, has been to alternate the observations, reading off first the distance of the free flame; 2d, that found with the plate of glass under experiment; 3d, free flame; 4th, plate of glass, &c.; and as the variations in the brightness of the lamps are generally slow and steady, by this mode, the error from this source is very greatly reduced.

Dove considers this form of photometer the only one which is capable of comparing the intensity of two masses of light having different colors; the degree of accuracy attainable in this direction can be judged of by the experiments detailed below. As the square spot will in such cases be differently colored from the ground, it always remains plainly visible, but there is a moment when the edges of this spot, to a great extent, disappear, and it seems to blend softly into the white ground. This I assume to take place when the illuminations are exactly balanced, and have accordingly employed it as the test in my experiments on colored media. Though the determinations with colored light are on the whole pretty satisfactory, still they by no means reach the accuracy easily attained when the two masses of light are similar in tint. With regard to the following observations, it may be remarked, that those made with col-

ored light show fully the degree of accuracy that can be attained in this direction with the photometer, the experiments, as far as they extend, being made with much care, though in such a way that each reading is entirely independent of the preceding, and has exercised no influence on that which follows.

To measure the amount of light reflected from a polished surface, I have used a plan not unlike that employed by Potter;[2] the slide, in the place of the lamp, carries the polished plate in an upright position and supported in such a way that it can be rotated on its axis and adjusted properly. Its axis corresponds with the centre of a divided quadrant by which the incidence of the ray can be measured. I is a shade to prevent the light from reaching the screen. The vernier is made to correspond with the perpendicular axis of the mirror, and of course the distance of the lamp's flame from the mirror's axis is always to be added to the reading obtained. By removing the shade I, and placing it so as to protect the mirror from light, the direct light of the lamp falls on the screen. As the lamp stands then a little obliquely, the slight error thus introduced must be allowed for, or, what is better, the lamp and mirror may be placed on either side of, and at equal distances from, the axis of the photometer.

3.



Prof. Silliman, Jr., pointed out to me some weeks ago, that small errors might be introduced by reflection from the walls of the room. This is guarded against by the use of several blackened shades properly disposed about the instrument, or by experimenting in a room with blackened walls, as is usual in determining the photometric power of illuminating gas.

Determination of the amount of light transmitted by a plate of colorless, polished crown glass $\frac{1}{12}$th of an inch thick:

Flame free.	With plate.
20·7 inches.	19·95 inches.
20·85 "	19·9 "
20·8 "	19·9 "

Amount of light transmitted, 91·09 per cent.

Determination of the amount of light transmitted by a plate of colorless glass, finely ground on one side, $\frac{1}{12}$th inch thick:

Flame free.	With ground glass.
20·2 inches.	13·1 inches.
20·35 "	12·8 "
	13·1 "
	13·05 "

Amount of light transmitted, 41·13 per cent.

[2] Physical Optics, p. 112.

Determination of the amount of light transmitted by one plate of polished orange colored glass, $\frac{1}{10}$th inch thick:

No. 1.		No. 2.		No. 3.	
Flame free.	Orange glass.	Flame free.	Orange glass.	Flame free.	Orange glass.
23·05	15·50	22·10	15·50	22·2	15·40
22·60	15·80	22·20	15·75	22·2	15·65
22·60	15·40	22·40	15·40	21·15	15·10
22·64	15·30	22·60	15·60	21·8	15·00
22·15	15·65	21·90	15·50	21·2	14·3
22·85	15·50				
Amount of light transmitted,	47·45		48·74		48·42 per cent.

Determination of the amount of light transmitted by a plate of deeply colored red glass, $\frac{1}{10}$th of an inch thick:

Flame free.	Red glass.	Flame free.	Red glass.	Flame free.	Red glass.
21·5	7·3	21·10	6·7	20·9	6·5
21·	6·3	21·15	6·6	20·2	6·7
22·	7·2	21·40	6·9		7·0
22·	6·7		6·8		6·2
	6·7		6·6		
Light transmitted,	10·03		10·05		10·36 per cent.

Peace Dale, R. I., May 7th, 1863.

ART. X.—*On Aerolitics, and the fall of Stones at Butsura, India, May*, 1861; by N. S. MASKELYNE.[1]

THE branch of science that treats of Meteorites has acquired sufficient importance to justify our giving it a special name, and I therefore propose for it the denomination with which this article is headed. Many reasons conspire to render this study of "aerolitics" one of increasing interest, and to make it highly desirable that collections of meteorites should exist to illustrate it, as complete as possible, not only in the numbers of the different falls they represent, but also as regards the modes in which the specimens are prepared for exhibition. These remarkable bodies will always command a general interest, from the fact that in them we see matter foreign in its origin and history to our own world, and handle, in them, the only tangible substances that belong to the space beyond our atmosphere. But the special interest attaching to a collection of them arises from the fact that, while they exhibit features of marked similarity, they withal, both

[1] Extracted from an article entitled *Mineralogical Notes*, by Professor MASKELYNE and Doctor VICTOR VON LANG, (of the British Museum) in the L. E. & D. Phil. Mag., No. 165.

as regards their constituent minerals, and the manner in which those minerals are mixed with each other, possess almost every one of them a very distinct individuality. Moreover, every day that the collection of specimens representing the older meteoric falls is deferred, adds to the difficulty of forming a complete series of them. It was on these accounts that the small but valuable collection that three years ago existed in the British Museum, has since that period been very largely increased. Towards the furthering of this object, most valuable assistance has been rendered by Governors of Colonies and Indian Presidencies, who have exerted their authority with a liberality that has been in one case, indeed, rivalled by the patriotism of a valuable and learned body, the Asiatic Society of Calcutta. The result of this, and of the considerable acquisitions made by purchase, has been that the aerolitic collection, which is an appanage to the Mineral Department in the British Museum, has now risen in point of material into the foremost place among such collections in the world.[3]

To accumulate so great a material is, however, but one step towards the end which should be held in view in the formation of a scientific collection. The next step consists in making that material available for the use of science, partly by a proper preparation and exhibition of the specimens, partly by a complete description of them. I propose in this and subsequent papers to contribute something towards the last of these objects.

Yet, when one approaches the subject with a view to undertake investigations in it, one cannot help feeling some disappointment, as well at the incompleteness of the chemical results that have been hitherto obtained, as at the unsatisfactory position of our knowledge concerning the origin and the sources of meteorites. Aerolitical science has to deal with the circumstances that attend the fall of a meteorite, no less than with its mechanical condition and its chemical composition; and from the data thus acquired it has to arrive at conclusions regarding the origin, the motion, and the cosmical relations of the foreign matter that thus wanders as it were into the atmosphere of our earth.

The general literature of the subject is becoming very considerable. Besides the tables and reasearches published by Mr.

[3] Every great collection has its own characteristic merits. If I may speak of that in the British Museum as the richest in material, taking the mass of the specimens as well as their numbers into consideration, it is with cordial pleasure that I express the highest admiration and respect for what I will not call a rival collection at Vienna. That collection is a classical one. Its specimens have been gradually collected, well described, and admirably exhibited. That aerolitics exists at all as a scientific subject is probably due to the existence of, and the care bestowed on, that collection. In the cause of science it is to be hoped that persons in authority in Vienna may not feel any jealousy of the rising collection in London, but may be ready to exchange, to the mutual advanatge of both collections, duplicate specimens of aerolites not common to the two.

Greg in our own country, and besides many papers of Baron Reichenbach in Poggendorff's *Annalen*, Hofrath Haidinger has, by his active pen and energetic mind, contributed, in Austria, perhaps more valuable notices on the fall of meteorites than all other living authors; and Dr. J. Lawrence Smith, as well as Prof. Silliman, by their accurate collection of facts and by their own investigations as chemists, have done much for the subject in America, where also the vigilant activity of Prof. Shepard has been conspicuous in collecting and distributing the specimens themselves.

The more special and exact literature, that, namely, which details the work done on meteorites in the laboratory, carries the names of the best inorganic analysts of this century, including those of Rose, Wöhler, and Rammelsberg. But if the progress thus far made in either the general or the special parts of the subject is not very large, it is at any rate enough to convince us that we see with tolerable clearness the questions to which we have to seek answers, and what are the cardinal points of interest raised by the presence of a meteorite on our globe, and by the circumstances attending its advent to it.

The chemical methods adopted for the analysis of a meteorite are probably unsatisfactory to every chemist who has employed them. The separation of the olivinoid and soluble felspathic from the insoluble felspathic, augitic, and other constituents, by the action of an acid, is necessarily incomplete; and the assignment of even empirical formulæ for the augitoid and felspathic ingredients is no less unsatisfactory. Yet in many meteorites it seems very difficult to conceive any better direct mode of operation. The intimate manner in which the different minerals are sometimes mingled, in what I may call a microscopic breccia, building the structure of the minute spherules in some of those belonging to the large group to which G. Rose has given the happy name of chondritic meteorites, and the excessive subdivision of the nickel-iron which in infinitesimal spangles is disseminated alike through homogeneous spherules and through those which present this brecciated character,—these facts, which the microscope alone reveals, seem to bar the chemist from any complete mechanical separation of the ingredients of many meteorites, whether by the agency of a magnet, or by that of the selection by the eye and hand, of distinct homogeneous particles.[3] Still there are cases in which analysis is possible; in some mete-

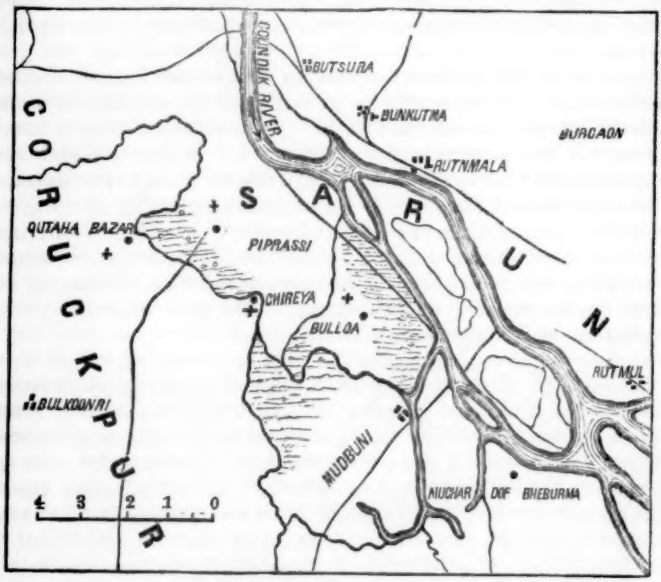
[3] Probably it would be found practicable to determine the iron indirectly by the estimation of the hydrogen developed by the treatment of the aerolite with acids, under conditions convenient for collecting that gas. The sulphuretted hydrogen might be estimated at the same time; and even if it were all calculated as emanating from meteoric pyrites, the ultimate error in the analysis would be less than by a method in which the entire separation of the metallic iron is generally impossible, and the estimation of ferrous oxyd therefore as often too high.

orites, as in that of Parnallee, the minerals are tolerably isolated from each other; and the fact, that the chemist, in dealing with such meteorites as those of Chantonnay, Stannern, Luotolax, Bokkevelde, and Bishopville, is enabled to place each of them as the characteristic member of a group, may furnish ground for the hope that approximate methods may be found for at least determining the nature of the minerals contained in any given meteorite. One such method appears to be furnished by the microscope. A thin transparent section of an aerolite exhibits, under a low power, in a tolerably characteristic way the minerals of which the aerolite is composed. By comparing these minerals as thus seen, and as observed *en masse* in the specimen, with the minerals that predominate in certain well-investigated and, so to say, standard types of meteorite, one soon learns to discriminate between them, and to predicate of any given individual aerolite, with what others it presents mineralogical analogies, though the assignment to each of these minerals of its precise place as a mineral species is in some cases very difficult. Occasionally, however, as in the coarser-grained varieties, one is enabled to discriminate and to separate by mechanical selection for chemical analysis certain mineral ingredients in a state of considerable purity.

I have sought by these means to determine the lithological character, if I may so call it, of some of the undescribed meteorites in the British Museum. As a nomenclature is much wanted in our language to represent what is so completely expressed by the terms Meteorstein and Meteoreisen in the German, I propose calling the former (the meteoric stone) by the original term Aerolite, the meteoric iron by the term Aerosiderite, and the intermediate varieties (including the Pallasites of Rose), in which the iron is continuous and associated with silicate, by the term Aerosiderolites or Siderolites. The term meteorite would remain a generic expression for the whole.

The fall of Butsura, May 12, 1861.—The group of aerolites that fell on May 12, 1861, on the banks of the Gunduk, forty-two miles northeast of Goruckpur, presents features of a general interest that claim for it a prominent place among those to be described in these Notices. Five pieces of that group were sent to the Asiatic Society of Bengal, at Calcutta; and they have been thence forwarded to London, where they were exhibited during the period of the International Exhibition, at the British Museum. They have since been cut, in directions agreed upon by Mr. Oldham on the part of the Asiatic Society, and in accordance with a liberal and patriotic resolution of the Society to share with the National Museum in London their valuable acquisitions in Indian aerolites. These five stones fell at four distinct places,

southwest of the main stream of the Gunduk, near the village of Mudbuni and on the opposite side of the river to Butsura, which, as being the nearest place indicated on the Royal Atlas of Johnston, is perhaps the best to give its name to the fall (see Map). The four spots where the aerolite fell are marked with a cross on the map, and form the angles of an irregular four-sided figure, one side of which runs nearly northwest and southeast, taking a direction parallel to the general course of this part of the Gunduk. The northern angle is very near to, and rather to the north of, a little place called Piprassi; the southeastern angle is a little to the northwest of one called Bulloah. These points are some three miles apart. Two very small fragments, weighing about 5 and 7 ounces respectively, fell at the latter locality. A thin slab-like piece fell at the former. It weighed about 11 pounds. Of the other angles, one is formed by a spot called the Qutahar Bazar (described in one account as in the Thannah of Nimboah); this is the northwesterly angle. The southern angle is at a spot called Chireya. The stones that fell at these two points respectively weighed 13 lbs. and 8¾ lbs.



These points, like the former, are some three miles apart; and whereas Chireya and Piprassi are only two miles, the northern point, Bulloah and Qutahar Bazar are some three miles distant from each other.

For the narrative of the circumstances accompanying the fall of these aerolites, I am indebted partly to Mr. Atkinson, the Secretary of the Asiatic Society, partly to my friend Dr. Oldham, the Director of the Indian Geological Survey.

The fall of the Qutahar Bazar and Chireya specimens was heralded by a report from out of a cloudless sky with a sound like that of ordnance, succeeded by several successive peals of seeming thunder. An appearance as of smoke was seen above the ground where they fell. One stone penetrated the soil for a cubit (=18 inches); the other did so to half that depth.

The two small fragments from Bulloah were accompanied by phenomena well substantiated by a near eye-witness. A native was taking his cattle to the water, when he was startled by three very loud reports, and saw in the air on high "a light" (a luminous body), which fell to the ground within 200 yards of him. Here too the sky was serene, and the weather fiercely hot, but there was a very small cloud, out of which this witness stated the report and the luminous body to have come. "First," he adds, "there was the loud report, and about the same time I saw the light like a flame; then the stone fell, and in falling made a great noise, and after it fell the sand was taken up high into the air." He went to the spot whence the sand had been raised from the ground, and found there five pieces of stone. They were very hot, and so was the sand all round, which was thrown up to the height of a foot. Unfortunately only two of these five fragments were preserved. Dr. Oldham further mentions that the incandescent fragments in falling are stated to have scintillated like iron when at a white heat.

The Piprassi stone was seen to fall by a witness quite independent of the other, but unfortunately from a much greater distance. In the midst of the calm hot day, while sitting in a field on the east side of the village of Piprassi, with many of the villagers, he states that they were startled by three loud reports succeeded by a rumbling sound which gradually died away. Their attention was immediately arrested by a cloud of smoke, which rose from the ground at about 1000 yards from them. They saw nothing like a falling body, but they heard a whistling sound as of a bullet, but much louder. They went to the spot and found the stone, round which the gravel had been thrown up for some 2 feet. Fortunately the stone was not carried away, for nobody touched it for two days. It was Mahadeo!

Two hours after the fall, the serenity of the weather was interrupted by a storm accompanied by a little rain.

The reports of the explosion were heard at a distance of sixty miles from the locality.

Dr. Oldham, on sending these most interesting aerolites to England, accompanied them by remarkable observations of his

own. The two little Bulloah fragments fit exactly together, and both fit on to the Piprassi stone. The Chireya stone in like manner fits with sufficient precision to that which fell at Qutahar. He surmised also that a careful adjustment would succeed in uniting all five fragments into a whole; and he indicated as a guide to this adjustment, a remarkable vein of iron which ran through the Piprassi and the Qutahar stones. I have since tried every possible means of effecting this; and though it is not practicable to find continuous surfaces of contact on the Piprassi and Qutahar stones, I have been enabled to determine the precise position they must have occupied relatively to each other, and have modelled and constructed an intermediate piece which, allowing contact of the stones at one part, builds the whole of the fragments into one large shell-like piece, obviously itself a fragment of some far larger mass. But this presents also another point of great interest. The Bulloah and the Piprassi stones, at the contact surfaces by which they fit together, exhibit no crust, though in other respects coated with it. The Chireya and Qutahar fragments, on the other hand, present a crust hardly, if at all, distinguishable from that covering the rest of their mass, on the very parts that form the faces of junction, and at which they fit with unquestionable precision. These surfaces indeed are smooth, and the edges very much rounded off, while those of the Bulloah and Piprassi stones fit together with the exactitude of adjustment with which the portions of a broken piece of oolite might be reunited.

Before attempting to draw conclusions from these facts, I will describe the general characters of the several fragments, in order that all the data offered by this aerolitic fall may be given in consecutive order.

The two that have been preserved out of the five stones that fell at Bulloah are small fragments, fitting on, as before mentioned, to one of the long edges of the Piprassi stone. Probably the whole five formed a long bar-like piece fitting on to that edge, and these two would, in that case, constitute the half of it.

The Bulloah stones are rounded along their summits and sides, and are there coated with a crust of a sooty black, and of dense texture. On the surface of contact they and the Piprassi are not crusted. The material of which the interior of the Bulloah stones is composed proves, when examined by a lens, to contain a profusion of protruding points of metallic iron. It presents a yellowish-brown ground-mass. It is mottled with irregular dark stains, which surround the metallic iron. This iron, associated with a considerable amount of meteoric pyrites, is present in this aerolite to a very high percentage. It is very evenly distributed in small, isolated, irregularly formed and sometimes crystalline-looking particles, not aggregated into a sponge, as in

the siderolites, but, as in the beautiful aerolite of Akbarpúr, the grains of metal seem linked by a ferruginous or iron-stained mineral, which may possibly indicate the vestiges of a sponge-like structure of the iron at some earlier period in its history, when perhaps the silicates were less basic than at present, and less of the iron oxydized.

Besides these ingredients, there are several very irregularly-distributed spherules of a mineral of the greenish-brown color and translucency, as well as the lustre, of dirty bees-wax. It is somewhat transparent in thin sections, and presents the characters of olivine.

A minute amount of iron pyrites occurs besides the meteoric pyrites; and a little of a very dark-colored mineral is also present, generally with a lustrous fracture, and perhaps occasionally somewhat crystalline.

In a section under the microscope with a power of one-inch focus, this aerolite does not prove to be a very remarkable one. The mass of it seems to consist of olivine. This is associated with a gray mineral, and also with one that is of an opake white. This gray mineral in some cases seems to constitute entire nodules of the aerolite, and sometimes seems mingled in the sort of brecciated mass, containing olivine crystals, that forms other nodules in it. It presents the appearance, in the former case, either of a dark mottled surface spangled with dark points (consisting sometimes of iron, and in some cases curiously distributed, as if spurted through the mass from a centre), or of a mineral presenting very regular and minute parallel cleavage-planes with dark gray bars running along them, often rayed out like a fan, and with cross-cleavages usually oblique, but at angles which vary with the inclination of the section to the axis of the crystal.

There is also another mineral, transparent and presenting cleavages nearly perpendicular to each other, which appears to be distinct from the foregoing.

What these minerals thus associated in small proportion with the olivine may be—whether they are solely augitic, or whether also the long feldspathic-looking bars are really fragments of some feldspar—is at present difficult to say with certainty. But in a subsequent article in these Notes I purpose giving all the data I possess for assigning to these and other meteoric minerals their true mineralogical character.

The Butsura fall, therefore, seems, like other aerolites rich in iron, to approach in character to a siderolite in that the silicates it contains consist, as I believe, for the most part of olivine. This olivine is generally very transparent, and comparatively colorless; but near the iron particles, and forming a continuous fringe to them, its granules become of a ferruginous color, and

are at times, especially in parts of the Qutahar and Chireya stones, red, like fragments of garnet or zircon.

The meteoric pyrites is present in a ratio of about one-half the apparent quantity to that of the iron. It is generally in little independent particles of the same average size as those of the iron; and it sometimes is continuous with the iron in the same particle, like the copper and silver of Lake Superior.

The Bulloah stone exhibits less of the ferruginous olivine than the others around the iron, and may perhaps contain more of the barred and grey mineral or minerals. The result is a paler hue on it. Its crust, on the other hand, is thicker and coal-black, that on the other stones having a browner cast.

But the specific gravity of the aerolite seems pretty constant in its different parts, namely about 3·60.

The next stone in order to the fragments that fell at Bulloah is the thin slab-like piece that fell at Piprassi. One of the faces of this piece is convex, while the other side presents a somewhat hollowed form. It is nearly rectangular in its general outline. The inner, as well as the outer side, presents some large but shallow hollows or "pittings." This piece, as before observed, does not fit on directly to the great mass that fell at Qutahar Bazar; but, that it formed a closely contiguous part to it on the original aerolite, there can be no doubt. In fact, the general contour of the stone, the correspondence of the outline and character of the shallow hollows on both, and, finally, the existence in them of the remarkable vein of nickel-iron before alluded to, and which runs persistently in one plane through each of them from the top to the bottom,—these all serve as guides to the restoration of the original form of the aerolite, so far as these two parts of it are concerned, and are the grounds of justification for the restoration of this part of the meteorite which I have attempted, by moulding the small intermediate piece, to unite the two stones. The Qutahar stone, which becomes thus adapted to the Piprassi piece, is a fine mass of an irregular wedge shape. The inner side is fitted with large shallow depressions, and presents a rather concave surface. The outer side is flat and smooth. The base on which it stands, and which is the result of the wedge-like form, is also smooth, rounded at the edges, and presenting hollows and irregularities on one half of its surface, while to the side of this base, on the inner or just below the concave part of the stone, the irregular piece that fell at Chireya adapts itself. This last fragment is somewhat pitted and deeply grooved on its upper side, and rounded everywhere else. Indeed, notwithstanding the precision with which it fits to the Qutahar stone, the faces and edges at the parts of contact are rounded off so as almost to obliterate the original form of the stones at this part. The contour presented by the reconstructed mass, so far as the reuniting

of these scattered fragments enables one to build it up, is that of a shell or the thick outer rind of one side of a considerable aerolite.

The lithological character of the Piprassi, Qutahar, and Chireya stones is very similar to that of the Bulloah pieces. But there are differences between them worthy of being noticed. Thus the crust on them is not very different from the dense black crust that coats those of Bulloah; it is, however, less characteristic and less thick. They are all dull, as the crusts on highly olivinous meteorites generally are, as contrasted with the shining enamels on the feldspathic-augitic kinds. It exhibits crystalline metallic-looking points, as well of iron as of meteoric pyrites and, at very rare intervals, of iron pyrites, that are disseminated among small globular projections of a pitch-black color. It is these black projections, on the other hand, that constitute the whole mass of the Bulloah crust. But in the three larger masses the crust assumes a dirty blackish-brown hue.

The facts above recorded appear to me to throw some light upon several interesting questions.

We may hazard a pretty safe conjecture as to the direction of the Butsura fall, by observing that the lighter stones fell to the S.E. of the heavier ones; the Bulloah three miles S.E. of Piprassi; the Chireya a similar distance E.S.E. of Qutahar. If we suppose that the disruption of all the stones was simultaneous, we might further assume that they fell with a diverging flight; for the Qutahar Bazar and Piprassi points are considerably further asunder than those of the Bulloah and Chireya falls. In fact, a line passing from the E.S.E. to W.N.W. would represent the direction of the flight of the aerolite; and if we are to judge by the different divergences of the stones, that flight would not have been at a great inclination to the horizon.

Had it been quite horizontal, the point of the divergence would have been, on this view, about seven miles E.S.E. of the central point of the fall, and two miles N.W. of the Mudbuni. As, however, it would seem to have fallen from a considerable elevation, it may have been much further off, though the point of disruption would have been somewhat nearly vertical over the position thus indicated.

But this fall is remarkable for the evidence it affords of the incrustation of an aerolite subsequently to its disruption, as well as of the probability of successive disruptions, of which one, at least, was not followed by incrustation. In the great Parnallee aerolite, and still more in that which fell at Bustee, we have cases, of which indeed every collection must exhibit some more or less evident examples, showing crusts on different parts of an aerolite that seem not to have been contemporaneous —where, in fact, the crust on one part has not the thickness and

homogeneity that characterizes that on another part. The following, in the British Museum collection, are cases in point: Stannern, Bokkeveldt, Benares, l'Aigle, and Mezö-Madaras. These facts are among those we have to explain. On the present occasion they were accompanied, according to every witness, by reports in the air, and by a subsequent roll of thunder. In two cases the distinct reports were three in number. There was a cloud in the sky, out of which the aerolite seemed to descend; while at Bulloah the stone or stones were seen to fall as a luminous body, which at some part of its path appeared to scintillate in the air. The shell-like form, too, of the united fragments, in suggesting the idea of an internal cone or mass from which the external pieces have been severed, recalls to mind the suggestion of Mr. Benjamin Marsh, that the bursting of the meteorite is the result of the expansion produced by heat. If we couple with Mr. Marsh's suggestion the remarkable explanation by Hofrath Haidinger of the intense coldness declared to have been exhibited by the Dhurmsala stones after their having fallen quite hot, I believe that suggestion will prove a very fertile one. The coldness of the cosmical space must be shared by bodies wandering therein without atmosphere.

Such a body entering with planetary velocity the terrestrial atmosphere, is arrested in its course with an abruptness of which it is difficult to get a clear conception as is that velocity itself. The intense heat instantaneously developed on the surface of the mass will assuredly be sufficient to melt that surface down into an enamel, before it could have time to penetrate to even a sensible depth into the body. If, as is probable, this fused and white-hot enamel flies off from the mass as it proceeds with the scream of a huge projectile through the air, its place will be continually taken by a fresh and continuously flowing stream of the same incandescent material. That material, too, is combustible. The metallic iron in many an aerolite ranges above 20 per cent, and is associated with sulphur as pyrites, and sometimes in other forms. Here at least is cause for much, if not a sufficient cause for the whole, of the spectacle exhibited by the blaze of a meteor. That the air itself is also heated to whiteness, and as such becomes visible, as Haidinger suggests, is highly probable, and would add still more to the brilliancy of the light.

But while the enormous velocity of the body is thus instantaneously arrested and converted into heat, the effect of that heat will not be exhibited in the molten spray of enamel alone. The heated surface will gradually, but by no means slowly, impart its heat to the interior; and notwithstanding the non-conducting character of the stony ingredients of an aerolite, the outer portions (a sort of shell around it) will rapidly rise in temperature.

The coldness of the interior would only gradually be overcome, and, long before it would be so, the expansion of the external parts would tend to tear them away from a contracted and far more than ice-cold core within. The limits and the form of that core, the conditions under which disruption would ensue (indeed, whether it would ensue at all, as it would not if the mass were absolutely homogeneous), would depend on the structure of the mass, its directions or planes of weaker aggregation, or perhaps the unequal distribution in it of matter of various degrees of conductibility. But when the disruption comes, it must come with explosion.

The process may be repeated, or it may take place at different intervals on the different sides of the meteorite. The earlier explosions may take place at points in its path where there is still velocity enough to produce a fresh enameling,—sometimes in a copious flow, at others only enough to barely glaze the exposed surface of the stone again; the later ones may occur when the velocity is more nearly spent, and the friction is no longer competent to generate the glaze.

The cloud in the air, out of which the meteorite has been seen to come in so many authenticated instances, would be satisfactorily explained by the dust of the enamel after its separation from the aerolite in its course, and the combustion of its iron, sulphur, &c.; perhaps, also, small fragments are splintered and fly off by the same principle as the larger ones, and, partially burning, becomes dust too.

Following in the track of the body, this dust would soon, however, linger behind it and hang in the air like a vapor-cloud, as is often seen to be the case in the wake alike of a meteor and of a meteorite.

Finally, if the reports represent the successive concussions of the air produced by the disruption of the aerolite (and reaching the ear generally in the inverse order of their occurrence in time), we must attribute the "thunder" that is so often described as succeeding the reports, to the echo of the reports themselves.

That a noise, the true extent of which is likely to be exaggerated, should be heard over so large a range of country as sixty linear miles, is perhaps not so surprising when we consider the distance to which a small cannon may be heard, even over a surface of country teeming with obstacles and air-currents calculated to impede the passage of the sound; whereas from a height of two or three miles in a still, clear air, the spread of even a comparatively small sound over an area with a radius of thirty or forty miles seems nothing astonishing. To me, at least, who have heard the roar of a train between Shrivenham and Swindon, as I stood, on a still night, in the station at Cirencester, a distance of certainly nearly twenty miles, such a wide promul-

gation of a sound in the air is no more difficult to understand than it is to credit the assertion of our aeronauts, who a few months back heard a musical instrument played on the earth when their balloon was some three miles above the ground. That this propagation of the sound of a cannon or a train is not due to the conduction of the earth, is proved by the fact that it is only in certain states of the atmosphere, independent of wind, that it occurs.

The cause to which I have assigned the disruption of a meteorite, and the reports which accompany it, may also furnish an explanation of the great size of some aerosiderites and siderolites, as compared with that of the largest stones. The more rapid conducting power of the metal, as well as its greater power of resisting a divellent force, would—perhaps after a first disruption—tend to prevent the repeated breaking up of the mass; and this may be the case in many instances, notwithstanding the fact that, in others, meteorites of this kind have fallen in associated and perhaps dissevered masses or even in showers.

Art. XI.—*The Sun and Stars photometrically compared;* by Alvan Clark.

If we place a lens of known focal distance, one foot for instance, between the eye and a star of the first magnitude, or one of any considerable brightness, with conveniences for guiding its movement in distance, to any point where it may be needed, and find the star just visible, or reduced to a sixth magnitude, when the lens, if a convex, is eleven feet from the eye, it becomes clear that, since the star has undergone a reduction of ten diameters, it would be visible, if removed in space to ten times its present distance. This, however, is on the supposition that no absorbing or extinguishing medium exists in space.

If a concave lens be employed, the measure must be commenced at the lens itself, but if convex, at the focal point; or once the focal distance must be subtracted from the measure, and the number of focal distances remaining corresponds to the number of reductions under which the object is viewed.

Castor is visible, when reduced,	10·3	times
Pollux,	11	"
Procyon,	12	"
Sirius,	20	"
The full Moon,	3,000	"
The Sun,	1,200,000	"

I have actually seen the sun under such a reduction; attended by circumstances which have led me to believe that it is about

the limit at which the naked human eye could ever perceive this great luminary.

I have an under-ground, dark chamber, 230 feet in length, one end terminating in the cellar of my work-shop, and the other communicating with the surface of the ground by a vertical opening, one foot square, and five feet deep. In a moveable partition, between this opening and the end of the chamber, a lens of such focal distance as I choose can be inserted. A twentieth of an inch focus I have employed, of the best finish possible; its flat side cemented to one face of a prism with Canada balsam.

No light whatever can enter the dark chamber, except through this little lens. A common, plane, silvered, glass mirror, placed above-ground, over the vertical opening, receives the direct rays from the sun, and sends them down into the prism of total reflexion, by which they are directed through the little lens into the chamber.

An observer, in the cellar, 230 feet distant, sees the sun reduced 55,200 times; and its light, in amount, varies but little from that of Sirius.

Upon a little car, moveable in either direction, by cords and a pulley, is mounted another lens, with a focal distance of six inches. The eye of the observer is brought into a line with the lenses, or so near it, that he sees the light through the six inch lens; then, by the cord, he sends the car into the chamber, to the greatest distance at which he can see the light, like that from a star of the sixth or seventh magnitude.

At noon, March 19th, with a perfectly clear sky, I found the sun visible through the six inch lens, when it was removed to the distance of 12 feet from the eye. The distance between the lenses being 218 feet, the reduction by the small lens, if viewed from the point occupied by the six inch lens, would be 52,320 times; and that again by the six inch, distant from the eye 12 feet, or 24 times its focal distance, is reduced 23 times; making the total reduction 1,203,360 times.

It becomes now an important matter to ascertain as nearly as possible the proportion of light lost, by and through the media above described; the looking-glass, the prism, and two lenses; though joining the little lens with balsam to the prism, it may be regarded as one piece.

I have only investigated by experiments with artificial lights; but I find, when the mirror is placed at the angle which the sun requires at the date above given, the difference in the distance at which a direct light, and the same light reflected, is brought to a *minimum visibile*, does not exceed one-eighth part of the entire distance, and could not reach one-seventh, when the prism and lenses were interposed.

Again; the image of the highly illuminated atmosphere, for some degrees about the sun, is admitted with the sun's direct light, through the little lens, to the dark chamber; and the light, thus augmented, is observed in contrast with a darkness greater than that of a clear nocturnal sky. The entire loss by reflecting and absorbing is manifestly so small, and the light of the sky in the immediate vicinity of the sun, so great, that I can readily believe the waste, in effect, is fully made up; especially when considering the absolute blackness of the ground, upon which the light, in the dark chamber, is projected; and I can find no reason to doubt that the sun would appear as a star of the sixth magnitude, or be only just visible to the unassisted human eye, even setting aside the idea of an extinguishing medium, if removed 1,200,000 times his present distance; and at 100,000 times his present distance, he would only rank as a pretty bright star, of the first magnitude; though his parallax would be double that imputed to any star in the whole heavens. If his intrinsic splendor generally proves to be less than that of those stars whose distances have been measured, we need not infer that it is less than the average of existing stars; for, in case of a diversity among them, bearing any proportion to that among organic bodies, on the face of the earth, or the planets of our system, where the numbers are so comparatively small, the *visible* stars, would, of course, exceed, upon the average, our sun; for, by the laws of perspective, the small ones would be lost to our view, at distances from which the brighter individuals would appear as conspicuous objects.

Such would be the case with telescopic magnitudes, as well as with those visible to the naked eye.

The number of stars visible, by aid of the more powerful telescopes, is far less, in proportion to the power of the instruments, than those visible to the unassisted eye, or with smaller telescopes.

This fact has given rise to the doctrine of an extinguishing medium in space; which is accepted by the most able astronomers as the truth, and has been the foundation of much ingenious reasoning.

Plausible or probable, as this appears, I see no difficulty in understanding that an exceedingly great diversity in the intrinsic brightness of the stellar orbs, promiscuously scattered through space, *might* result in the same appearances as those on which this doctrine is founded. For, at the smaller distances, we should see the whole, both great and small, when using only moderate powers; but in the regions bounding the remotest reach of the great telescopes, though the great and the small might be there, it would be only the great that we should see; and those only as the most minute specks of light that can be imagined.

The vast number of smaller, or more moderate lights, like our sun, which may remain concealed among those of extraordinary splendor, yet so remote as only just sensibly to impress our vision when aided to the utmost that human skill can do, will be better understood when we consider the ratio in which an increase of radius increases the cubic contents of a sphere.

Upon the outer limits of such a sphere as would embrace the great mass of telescopic stars, a moderate depth, extended round the whole, would afford an immense amount of room for stars of all imaginable sizes. I desire to be particularly understood, that it is in those very remote regions, or beyond where any telescope, now in use, can possibly show stars of the average, or smaller sizes, that we may look for the modification introduced, by such supposed diversities, into the investigation of this doctrine of an absorbing medium.

Were all the stars in existence of one pattern, one uniform brightness, scattered broadcast through all space, I think the great telescopes would count up more nearly the numbers belonging theoretically to their powers than they now do.

However, with these suggestions, I leave this interesting branch of my subject for the present.

The ratio, in which the light from a celestial object diminishes with an increase of distance, needs no explaining; and I will close by briefly giving, in tabular form, my own results, with those published by Mr. Bond of the Harvard College Observatory, and by Dr. Wollaston, in vol. cxix of the *Philosophical Transactions of the Royal Society of London*, of comparisons between the bright star α Lyræ and the sun.

To bring the magnitude of our sun to an equality with that of this star, his distance would require to be increased, according to

Wollaston,	nearly	425,000 times.
Bond,	"	155,000 "
Clark,		102,000 "

The light received from these luminaries differs, according to

Wollaston,	as	180,000,000,000 to 1
Bond,	"	24,000,000,000 "
Clark,	"	10,400,000,000 "

I have alluded to the light in the atmosphere about the sun, as giving an increase to his photometrical force; though I am aware that such must be the case with a star; and it must bear the same proportion to the star's light, that it bears to the sun's light.

The difference, in effect, is here; we have several thousand stars playing into our atmosphere at once; but only one sun.

If the distances imputed to several of the stars, from parallax, can be true, I am sure, those having the taste, talent, and leisure, necessary for following up photometrical researches with effi-

ciency, cannot fail to find our glorious luminary a very small star; and to the human understanding, thus enlightened, more than ever, must the heavens declare the glory of God.

P. S.—Since the above had left my hands for the press, I prepared a close covering for the vertical opening to my dark chamber, with a circular perforation, subtending at the prism an angle of 32′; and substituted for the little lens one having a focal distance of one-eighth of an inch. By this arrangement, with the mirror placed above, and an eye-hole by its side, the sun light would be directed upon the prism, or just beside it, at pleasure.

I assumed that when the pencil was made to fall entirely outside of the lens, I was viewing a portion of the sky just equal in form and area to the sun itself, close by its side.

Allowing the direct light to pass centrally over the prism, I found the image visible for more than one minute after the last direct ray from the sun had left the line of the lens, although reduced nearly 22,000 times.

After proceeding thus far, it appeared to me, that could the sun be reduced to a *minimum visibile*, *without* reflexions, and the lenses so arranged that both eyes could be employed in observing, the results would be more satisfactory.

By removing the object-glass, eight inches in diameter, from the tube of my equatorial, and placing a lens in the eye-tube, one-twentieth of an inch in focal distance, and turning the eye end toward the sun, with the eyes 100 inches from the lens, I obtained such a view as the sun would present if removed 2,000 times his present distance. To accomplish the further necessary reduction, I applied an extension sliding tube carrying a lens one-thirty-fourth of an inch focus.

To my surprise the sun was visible when the distance between the lenses was such as to give a reducing power of two millions. But, upon examination, it appeared that light was copiously reflected upon the lens nearest to the eye, from the inner surface of the bright brass extension tube. After diaphragming and properly darkening this tube, on some occasions of very clear skies, a power of nearly 1,600,000 was required to send the sun entirely out of sight. Were not the use of both eyes, the avoidance of reflexion by the looking-glass and prism, and the increased altitude of the sun sufficient to explain the difference between this and the dark chamber observations, I might add, the angle of aperture of the lens nearest the sun is 80° and used without a screen, while only a much smaller proportion of the bright sky was sent down by the mirror into the prism. Seated comfortably upon the observing chair, with the driving clock acting, the face in the mouth of the tube nearly to the ears, and

a folded blanket drawn over the head, so as to exclude all light, I usually gaze listlessly into total darkness for about two minutes, that the pupils may expand, and the eye become prepared for its work. I then slowly remove or raise the blanket upon one side for the admission of light, barely sufficient to show the direction of the lens. A curious effect is here witnessed. Though no view of the sun had previously been obtained, and the eyes had wandered perhaps many degrees, they are suddenly arrested by the appearance of a good plump sixth magnitude star.

On closely drawing the blanket again, it fades to the faintest perceptible point, or disappears altogether, and the eyes wander from its place in spite of every effort to keep them upon it. But when the slightest ray is again allowed to enter past the head, it is reflected from the lens, and combining with the light transmitted from the sun becomes conspicuous as before.

This I have found to be the case when neither the transmitted nor the reflected light alone could stimulate the visual organs to a recognition of its presence.

Since noticing this fact, I invariably close every avenue to the admission of light about the head, preparatory to the final effort, and if the sun is not seen within five minutes, I give it up, reduce the distance between the lenses about one-twentieth, and try again. My eye gains about its maximum power for such purposes in three minutes. I am on my guard against the consequences of allowing insufficient time. When a view is obtained estimated equal to a faint sixth magnitude, the distance between the lenses is measured, and the reducing power computed. I do not, it will be seen, trust to any comparisons with artificial lights as standards, but make a *minimum visibile* the standard in all cases; which leaves the eyes and attention free to pursue one object at a time. A movable brass plate, with a perforation one-tenth of an inch in diameter, is placed nine inches beyond the lens next the sun, for admitting his direct rays, with only a very narrow annulus of surrounding skylight.

The following results were obtained April 28.

$6^h\ 30^m$ A. M.	1,055,360	sky admitted.
6 40	783,100	sky screened off.
12 noon,	1,308,000	sky screened off.
12 10 P. M.	1,574,400	sky admitted.

These morning observations were made for the benefit of a friend, who wished them for a special purpose.

The numbers in the noon observation are very near the maximum exhibited in many extreme efforts, made when the skies were remarkably clear.

It makes an enormous difference, it will be noticed, whether the sun is observed with the atmospheric light screened off or not; but, to give the sun and the star equal conditions, the eye should in both cases be shielded against the light from surrounding regions. If we could condense into a compass of less than the fourth of a second all the sky-light within 40° of a faint star, and add it to the star, it would give a manifold increase to its brightness. This is what occurs in observing the sun without the screen; though the atmospheric light in question is, by day, from the sun alone, but by night, from the host of stars of all magnitudes; which would make the conditions monstrously unequal; and this indicates the importance of clearing it away in both cases as effectually as possible. The method by which it is cleared from the sun is already explained; but for dealing with a star, I remove all the lenses from the finder tube to my equatorial, and place in each end of it, a disk of brass, with a central perforation, one-fifth of an inch in diameter, while another similar disk is placed upon an arm extending beyond the object end of the main tube, ten feet from the eye, upon a direct line with those in the finder. When a star occupies a given point in the field of the telescope, it is also in a line with the centres of these holes. But the plan is faulty; inasmuch as it is going back to the use of one eye; and since the holes must equal in diameter its expanded pupil, they admit sufficient light from the blank sky to be seen, if a sharp effort is made; though when that from a star of the seventh magnitude is brought in with it, an augmentation is quite apparent, and a bright sixth magnitude, like 32 Bootis, is instantly and constantly seen with the single eye, through the screen, with two pieces of plate glass interposed. Mitchell has estimated, from Bouguer's observations, that the sun would equal a star of the sixth magnitude, if he were removed to ten millions of times his present distance, which is nine times as much as could possibly be required, so far as I can understand by these experiments, to give him companionship with the star 32 Bootis.

Cambridgeport, May 26, 1863.

ART. XII.—*On Glucinum and its Compounds;* by CHARLES A. JOY, Professor of Chemistry in Columbia College, New York.

THE distinguished crystallographer, Haüy, having discovered a perfect identity in crystalline form, hardness, and specific gravity between the minerals beryl and emerald, requested Vauquelin to subject the former to a careful analysis. Beryl had previously been analyzed by Bindheim in 1790, with the following result:

$SiO_3=64$, $Al_2O_3=27$, $CaO=8$, and $Fe_2O_3=2$. Total 101.

Vauquelin[1] fused 100 parts of finely pulverized beryl with 300 parts of caustic potassa, and separated the silica (69 p. c.) in the usual way. He then precipitated the earths by carbonate of potassa, and digested the precipitate in caustic potassa, by which a portion, amounting to nine per cent, was again thrown down.

This property of re-precipitation from caustic potassa, in one portion of the beryl, attracted his attention, and the fact that he could not obtain an alum with it, when its sulphate was mixed with the sulphate of potassa, lead to the final discovery of glucina.

Vauquelin did not give a name to the new earth, but left it to his colleagues to propose one. In consequence of its forming salts of a sweetish taste, they called it *glucina*, from γλυκύς, sweet, γλυκύ, sweet wine, γλυκαίνω, to render sweet. The German chemists, however, have preferred the name *berylla*, from the mineral in which it was first found.

Since the days of Vauquelin, a number of minerals containing glucina have been added to the list. The following catalogue of these minerals, together with the literature of the subject, is believed to be tolerably complete.

1. *Alexandrite*—same as chrysoberyl.

2. *Alvite.*—Dana, Sup. iii, 5. Nyt. Mag. f. Nat., xiii, D. Forbes and T. Dahll.

3. *Aqua-marine.*—Vauquelin, Ann. de Chim. et Phys., [1], xxvi, 155. Hermann, Ann. de Chim. et Phys., [2], xix, 361. Don Pedros, magnificent specimen of, British As. f. Adv. Sci., i, 86. Du Menil, Schwgg. J., xxxiv, 454. Dana, Min., 178. Rammelsberg, Hdb. d. Min. chem., 553. Hausmann, Min., 603, 887.

4. *Beryl.*—Plin. Hist. Nat., xxxvii, 5, s. 20. Hard., ii, 776. Irenæus contra hæreses Ed. Ren. Massuet., 1710, Lib. i, proem. § 2, p. 2. Theophrast. de lapid., §§ 44, 45, 46. Klaproth, Beiträge, i, 9, iii, 215. Werner, 40, 41. Haüy, Traité, ii, 504. Gren., J. d. Phys., ii, 421, 1795. Graf v. Veltheim, Sammlung einige Aufsätze histor. antiquar. Miner. u. Abnl. Inhalts., 1800, ii, 134. Carl Ritter, Erdkunde, i, Africa, 2 A., 673-677. Beckmann's Beitr. z. Gesch. d. Erfind., iii, 297. Wilken, Gesch. d. Kreuzzüge. Beil. 8. Du Menil, Schwgg. J., xxxiv, 454; id. xxxix, 487. Fusion of beryl, Schwgg. J., xviii, 237; id., xix, 320. Apatite mistaken for beryl (απατάω, to deceive). Gilbert, J. d. Phys., xvi. 126 and 250. Hausmann, Handb. d. Min., ii, 603. Von Leonhard, Handb., 391. Beud. Traité, ii, 41. Phillips, Min., Brooke and Miller, p. 336. Ausserord, Beilage zur Augsb. Allgem.

[1] Ann. de Chim., [1], xxvi, 155, Feb. 15, 1798.

Zeitung, 1844, No. 347. Brugman, chromoxyd in beryl magnetic. Gilbert, J. d. Phys., iv, 33. Romé de l'Isle, Cristallogn., ii, 245. Blumenbachii spec. hist. nat. antiquae artis oper. illustrat., 1808, p. 31. Berzelius and Gahn, Schwgg. J., xvi, 265; id., xviii, 237. Berzelius and Klaproth, Schwgg. J., iv, 66. Borntträger, Leonh. u. Bronn's Jahrb., 1851, 185, and Liebig u. Kopp, Jahrb., 1851, 779. Breithaupt (Plattner, Lampadius), J. f. p. Chem., x, 249. Ditto, Schwgg. J., lx, 422. Hofmeister, J. f. p. Chem., lxxvi, 1. Kopp, Jahresb., 1859, 778. Lewy, Ann. Chim. Phys., [3], liii, 5. Mallet, Am. J. Sci., [2], xvii, 180. Mayer, Leonh. u. Bronn's Jahrb., 1851, 674. Liebig u. Kopp, Jahrb., 1851, 779. Moberg, Acta Soc. Scient. fennic., ii, 71. Berz. Jahrb., xxiv, 313. Müller, J. f. p. Chem., lviii, 180. Scheerer, Pogg. Ann., xlix, 533. Schlieper, Ram. Handb. d. Min., 555. Schneider, id., 555. Thomson, Outlines of Min., i, 399. Vauquelin, Ann. de Chim. et Phys., [1], xxvi, 155. Gilbert, Ann., xvi, 250. Jour. des Mines, No. xxxviii, 97; No. xliii, 563. Moore's Ancient Mineralogy, 146-150. Dana's Min., 178; 2d Sup., 4; 4th Sup., 112; 5th Sup., 403; 9th Sup., 6. Am. J. Sci., i, 242; ib., iv, 39; ib., vi, 222; ib., xviii, 291; ib., xl, 401; ib., [2], xiii, 264; ib., [2], xvii, 78. Von Kokscharow, Min. Russ., ii, 356. Zippe, Geschichte d'Met., 558. Sir David Brewster, Phil. Trans., 1819. Gilbert, Ann., lxv, 5; ib., lxix, 1; ib., lxix, 535. C. G. Gmelin, Pogg. Ann., l, 175. Awdejew, Pogg. Ann., lvi, 101. H. Rose, ib., lix, 101. Salm Horstmar, Pogg. Ann., lxxxvi, 145. Hermann, Ann. de Chim. et Phys., xix, 361; ib., xliv, 27; ib., lxii, 284; ib., xl, 109; ib., lix, 178. Journal des Mines, Jan., 1812. Gilbert, J. d. Phys., xliii, 110. Schwgg. J., xxxi, 261; ib., xxxiv, 454. Cleaveland's Min., 274–278. Kopp, Gesch. d. Chem., iv, 68. K. K. Russ. Gesel. d. Min., i, 343, 1842.

5. *Chrysoberyl.*—Haüy, Traité, ii, 303. An. de Mus. d'Hist. Nat., 1811, xviii. Gilbert, Ann., xli, 53, from Haddam, sent to Haüy by Mr. Bruce of New York, 1810. Hausmann, Min., ii, 430. Henry Seybert, Trans. Am. Phil. Soc., March 5, 1824, p. 116. Schwgg. J., xlii, 228. Thomson's Outlines, i, 400. Awdejew, Pogg. Ann., lvi, 118. Ann. Chem. Pharm., xliv, 270. Plinius, xxxvii. Hard. ii, 776. Klaproth, Beit., i, 97. G. Rose, Pogg. Ann., xlviii, 570. G. Rose, Reise nach dem Ural, ii, 379. Pott, K. K. Gesl. f. d. gesam. Min. St. Petersburg, i, 116. Werner, 5. V. Leonhard, Handb., 539. Beud., Traité, ii, 145. Mohs, Phys., 342, fig. 37. Damour, Ann. Ch. Phys., [3], vii, 173. Pogg. Ann., lix, 120. Dana's Min., 122. Moore's Anc. Min., 151. Jameson's Min., i, 202. Arfvedson, Vet. Acad. Hand., 1822. Schwgg. J., xxxviii, 4. Bergemann, Dissertat. Göttingen, 1826. Artificial, Caron and Deville, Ann. Chem. Pharm., cviii, 57. Rose, ib., lxiv, 287. Ebelmen, ib., lxviii, 265; ib., lxxx, 207. Am. J. Sci., [1], ii, 240; ib., iv, 37. Haidinger, Pogg. Ann., lxxvii, 228. Schwgg. J., l, 329; ib., li, 251. Cleaveland, Min., 204.

6. *Cymophane*—same as chrysoberyl.

7. *Davidsonite*—same as beryl, Phil. Mag. [4], xii, 386. Ann. Chem. Pharm., xix, 154.

8. *Emerald.*—Haüy, identity of emerald and beryl, Ann. Chim. Phys., xxvi, 156. Vauquelin, Ann. Chim. Phys., xxvi, 264. Am. J. Sci., [1], ii, 354. Klaproth, Beitr., iii, 215. Chemical Essays, London, 1801, 176. Moberg, Acta. Soc. Sci. Fenn., ii, 81. Ville, Comp. Rend., xli, 698. Lewy, ib., xlv, 877. Ann. Chim. Phys., [3], liii, 51. Rép. Chim. Appl., 1858–59, 27. Du Péron, Ann. Chim. Phys., xxiii, 68; ib., xiv, 64. Dumas, Séances de l'Acad., 1855. De Senarmont, ib., 1859. Dana's Min., 178. Cleaveland's Min., 274. Hausmann's Min., 603.

9. *Euclase.*—Vauquelin, J. des Mines, No. 28, 258. Haüy, Traité, ii, 528. Werner, 39. Beud, Traité, ii, 32. Mohs' Phys., 351. Weiss, Verhand d. Gesells. Natf. F., Berlin, 1820, 110. Weiss, Abhd. k. Akad. d. W., Berlin, 1841, 249. Berzelius, Schwgg. J., xxvii, 73. Schabus, Monograph. Wien Ak. d. W., 1852. Pogg. Ann., lxxxviii, 608. Kokscharow, Pogg. Ann., ciii, 348. Bulletin de l'Ac. de St. P., xvi, 284. Material zu Miner Russ., iii, 131. Hausmann, Handb. d. Min., 601. Von Leonhard, Handb., 395. Levy, Edin. Phil. J., xiv, 129. Mallet, Phil. Mag., [4], v, 127. J. f. p. Chem., lviii, 447. Damour, Comp. Rend., xl, 944. J. f. p. Chem., lxvi, 154. Dana, Am. J. Sci., xvi, 96. Ann. Chim. Phys., xi, 216; ib., xii, 26. Dr. Brewster, Edinb. J., ix, 217. Gilbert's J., lxix, 1. Schwgg. J., xxxiii, 106. Zinken, Schwgg. J., xxvi, 372; xxvii, 73. Rammelsberg, Hdb. d. Min. Chem., 570. Cleaveland's Min., 278.

10. *Gadolinite.*—Berzelius, Schwgg. J., iv, 51; ib., xxiii, 194; ib., xxvi. Afhandl. i. Fys., v, 54. Thomson, Steel and Richardson, Phil. Mag., vii, 430. J. f. p. Chem., viii, 44. Schwgg. J., viii, 450. Thomson's Outl., i, 410. Ekeberg, Ann. Chim. Phys., xliii, 228. Gilbert's Ann., xiv, 247. Scheerer, J. d. Chem., iii, 187, 1794. Mém. de l'Akad. de Stock., 1797. Dana's Min., 211; 4th Sup., 119; 6th Sup., 351. J. f. p. Chem., lxxiv, 271. Von Kobell, Schwgg. J., 1834. Pogg. Ann., ciii, 314; ib., lix, 101. Berlin, Berz. Jahr. B., xvii, 220. Schwgg. J., xiv, 33; ib., xvi, 404; ib., xxi, 261. Connel, Edinb. n. Phil. J., June, 1836. Ann. Chem. Pharm., xlviii, 224. Pogg. Ann., li, 414; ib., lvi, 479. Klaproth, Beit., iii, 52; ib., v, 173. Ann. Chim. Phys., xxxvi, 143; ib., xxxvii, 87; ib., xliii, 278; ib., xlix, 124; ib., lxii, 208; ib., [2], ii, 411; ib., [2], iii, 26. Brcht. d. k. Akad. d. W., Berlin, 1801, p. 16. Hausmann, Min., 542, 1590. Geyer, Crell. Ann. d. Chem., 1788. Descloizeaux and Damour, Ann. Ch. Phys., [3], lix, 357. Cleaveland's Min., 205. Orthite and Gadolinite, Pogg. Ann., lix, 103. Berz. Jahrb., xxiv, 318; ib., xxv, 365.

11. *Goshenite*—same as beryl.

12. *Helvine.*—Vogel, Schwgg. J., xxix, 314. C. Gmelin, Pogg. Ann., iii, 53. Rammelsberg, Pogg. Ann., xciii, 453. Breithaupt, Gilbert's Ann., lxiv, 42. Dana's Min., 194; 1st Sup., 9. Hausmann's Min., 870. Werner, 23. Triesleben's Beiträge, i. Haüy, Traité, ii, 168. V. Leonh. Hab., 462. Beud., Traité, ii, 168. Mohs' Phys., 397, fig. 206.

13. *Leucophane.*

14. *Melinophane.*—Th. Scheerer, J. f. p. Chem., lv, 449. Greg, Phil. Mag., [4], x, 510, 1855. Descloizeaux, Ann. Chim. Phys., [3], xl, 76. Rammelsberg, J. f. p. Chem., lxviii, 245. Pogg. Ann., xcviii, 257. Berzelius's Jahrb., xxi, 168. Dana's Min., 182; 2d Sup., 13; 4th Sup., 121. A. Erdmann, Vet. Acad. Hand., 1840, 191. Awdejew, Ann. Chem. Pharm., xliv, 270. Phillips's Min., 356. Hausmann's Min., 888. Rammelsberg, Handb. Min. Chem., 763.

15. *Phenacite.*—Hausmann, Handb. d. Min., 538, 1590. Nordenskiöld, Pogg. Ann., xxxi, 57. K. v. Ac. H., 1823, 1860. Beirich, Pogg. Ann., xxxiv, 519; ib., xli, 323. Ann. Chem. Pharm., xvi, 251. Mohs' Phys., 353. R. Hermann, Bullet. Soc. Imp. Mos., 1844, iv, 877. Hartwall, Ann. Chem. Pharm., xiv, 82. Berz. Jahrb., xiii, 157. Pogg. Ann., xxxi, 57; ib., cxxxii, 120. Ann. Chem. Pharm., xvi, 251. Phil. Mag., [4], iii, 378. Dana's Min., 189. Am. J. Sci., [2], xvii, 78. G. Rose, Pogg. Ann., lxix, 143. Rammelsberg, Hdb. Min. Chem., 553. Awdejew, Ann. Chem. Pharm., xliv, 270. Deville, artificial, Ann. Ch. Pharm., cxx, 178.

16. *Smaragd*—same as beryl.

17. *Tyrite.*—Phil. Mag., [4], xiii, 91. Dana's 4th Sup., 129.

As my object was to procure a supply of glucina, a re-examination of the above minerals was not deemed necessary. One hundred pounds of fragments of beryl were obtained from Acworth, New Hampshire, and ground in a gold-quartz mill, and decomposed according to the following methods.

Methods for the decomposition of beryl.

1. By passing chlorine gas over a calcined mixture of lamp-black, oil, and beryl.

2. By treating beryl with concentrated hydrofluoric acid and sulphuric acid.

3. By digesting seven parts of beryl and thirteen parts of fluor spar in eighteen parts of concentrated sulphuric acid.

4. By fusing beryl with three parts of fluorid of potassium, and digesting in sulphuric acid.

5. By fusing beryl with fluorid of ammonium, and digesting in sulphuric acid.

6. By digesting fifty parts of beryl in thirty parts of sulphuric acid, and fusing with one hundred parts of ferrocyanid of potassium and seventy parts of chlorid of sodium.

7. By fusing beryl with fluor spar.

8. By fusing beryl with half its weight of caustic lime.

9. By fusing beryl with litharge.

10. By fusing beryl with binoxyd of manganese.

11. By fusing beryl with three parts of carbonate of potassa and two parts of carbonate of soda.

12. By fusing beryl with two parts of carbonate of potassa.

1. *By chlorine gas.*—Finely pulverized beryl was intimately mixed with lamp-black and linseed oil, and calcined. Chlorine gas was passed over it at a red heat in a porcelain tube, and the more volatile chlorids of silicon and iron driven into the receiver. The chlorids of glucinum and aluminum were collected in the further end of the tube. The beryl was completely decomposed. An unsuccessful attempt was made to take advantage of the difference in the points of volatilization of the chlorids of iron, silicon, aluminum, and glucinum, in order to separate them.

2. *By hydrofluoric acid.*—This well-known method was tried for comparison, and, where the beryl was finely pulverized, was entirely successful. The glucina was separated from the alumina by carbonate of ammonia.

3. *By fluor spar.*—This method was proposed by Scheffer.[3] Seven parts of beryl, thirteen parts of fluor spar, and eighteen parts of concentrated sulphuric acid were gently heated in a leaden trough under constant stirring, for two hours, and then transferred to an iron vessel, and heated sufficiently to expel the fluorid of silicon and the excess of sulphuric acid. The decomposition is fully accomplished in this way, and the only objections to it are the presence of so much sulphate of lime in the solutions, and the difficulty in expelling the excess of sulphuric acid.

4. *By fluorid of potassium.*—One part of beryl was fused with three parts of fluorid of potassium, and digested in sulphuric acid. If this flux could be obtained in sufficient quantity, the method would be preferable to all others, as the mass fuses easily, the fluorid of silicon is driven off at a gentle heat, and the potash alum crystallizes readily, carrying down all of the alumina, thus at the same time accomplishing the decomposition of the silicate and the separation of the alumina and glucina.

5. *By fluorid of ammonium.*—Four parts of beryl were intimately mixed with nine parts of fluorid of ammonium, gently heated in a capacious platinum crucible, and fused at a low red heat. The mass was covered with an excess of sulphuric acid,

[3] Ann. Chem. Pharm., cix, 144.

and evaporated, care being taken to prevent the formation of nearly insoluble fluorid of aluminum by too great heat.

This method[3] for the analysis of silicates is one of the best ever proposed, and, as fluor spar is abundant, there is no reason why it should not be frequently applied.

6. *By ferrocyanid of potassium.*—The method of Corbelli,[4] for obtaining aluminum from its compounds, was applied to beryl. Fifty grammes of beryl and thirty grammes of sulphuric acid were digested for two hours, and the heat raised to 500° C. After cooling, one hundred parts of dry ferrocyanid of potassium and seventy parts of chlorid of sodium were added, and the mass exposed to the highest heat of an anthracite fire. The result was a button of iron, but no glucinum. The beryl was only slightly decomposed. A trial with an alumina salt also yielded a button of iron, but no aluminum.

7. *By fusing with fluor spar.*—One hundred and eight parts of beryl and one hundred and sixty parts of fluor spar fused very readily, but required close attention to prevent the mass from running through the iron crucible. The complication in the separation of the earths, occasioned by the introduction of so much lime, was found to be inconvenient in this method.

8. *By caustic lime.*—Two parts of beryl and one part of caustic lime were fused in a hessian crucible. The mass melted readily, but, as in the previous example, required care to prevent it from running through the crucible. This method[5] is rapid, requires only a low heat and cheap material, and would be highly advantageous, if suitable crucibles could be provided. I modified it somewhat, as follows: by fusing one part of lime, two parts of beryl, and three and a half parts of gypsum. (Two and one and a half parts of gypsum were tried.) This readly decomposed the beryl, forming a beautiful glass, and did not run through the crucible; but so much lime in the solution was an objection.

9. *By litharge.*—One hundred grammes of beryl were intimately mixed with three hundred grammes of litharge in an iron crucible, and gradually heated to fusion. The mass was then stirred with an iron spatula, and poured upon a marble slab. The cold slag had a dirty yellow color, and was soft and easy to pulverize. It was reduced to a fine powder, moistened with water, digested in an excess of nitric acid, and evaporated to dryness. The silica was separated, and the filtrate was left until nitrate of lead had crystallized out. The balance of the lead was separated by sulphuric acid, and the requisite amount of sulphate of ammonia then added to form an alum with the sulphate of alumina. This method with litharge proved to be

[3] Rose, Pogg. Ann., cviii, 19.
[4] Rep. pat. inv., Oct. 1858, 300.
[5] Debray, Ann. Ch. Phys., [3], xliv, 5.

admirable, and is highly recommended for the decomposition of other silicates. The yield of glucina was not quite equal to that obtained for the fusion with carbonate of potassa.

10. *Binoxyd of manganese.*—Two parts of finely pulverized beryl were intimately mixed with three parts of binoxyd of manganese, and exposed to the highest heat of an anthracite coal furnace for two hours. The mass fused completely, affording a dark glass resembling obsidian, hard and difficult to pulverize. It was digested in concentrated sulphuric acid, and the silica separated as usual. The beryl was fully decomposed. My object was to see whether the binoxyd of manganese would decompose silicates in this way, and, further, to try if an alum with the *protoxyd* of manganese ($MnO, SO_3 + Al_2O_3, 3SO_3 + 24HO$) could be formed. No such alum was obtained, thus confirming previous experiments in this direction.

11. *Carbonates of soda and potash.*—By fusing beryl with a mixture of two parts of carbonate of soda and three parts of carbonate of potassa, the mineral is easily decomposed, and, where the earths are separated by carbonate of ammonia or chlorid of ammonium, the method is very convenient. It has no advantages, however, over the succeeding and last method.

12. *By carbonate of potassa.*—One part of finely pulverized beryl and two parts of carbonate of potassa fused very readily in a platinum crucible. It will be seen, under the head of the separation of alumina and glucina, that this method was preferred to any other.

All of the above methods, and numerous others which it is not necessary to recapitulate, were subjected to repeated trials in my laboratory, and a large supply of glucina obtained for use in the further prosecution of this investigation.

Methods for the separation of glucina and alumina.

1. Chlorid of ammonium.
2. Carbonate of ammonia.
3. Caustic potassa.
4. Sulphurous acid.
5. Carbonate of baryta.
6. Hyposulphite of soda.
7. Decomposition of nitrates.
8. Acetate of soda.
9. Fusing with caustic potassa.
10. Formate of ammonia.
11. Decomposition of sulphates.
12. Potash alum.

1. *By chlorid of ammonium.*—The oxyds of iron, alumina, and glucina were precipitated by ammonia, and the precipitate digested in a concentrated solution of chlorid of ammonium, with constant replacement of the evaporated water. The iron and alumina, being insoluble in sal-ammoniac, are collected upon a filter, and the glucina precipitated from the filtrate by sulphid of ammonium.

This method, proposed by Berzelius,[6] was regarded by Wee-

[6] H. Rose, Handb. d. Analyt. Chem., ii, 61.

ren[7] as preferable to any other. My observations confirm the accuracy of the results to be obtained, if all of the precautions are observed, but the time required for the digestion of the mixture and the care to keep it at a proper concentration render it more tedious than other methods, without a corresponding increase in the yield of glucina.

2. *By carbonate of ammonia.*—The filtrate from the silica was dropped, with constant stirring, into a warm concentrated solution of carbonate of ammonia in excess, which precipitated the alumina and dissolved the glucina. The solution was left for some days in a corked flask, and occasionally well shaken. After separating the alumina by filtration, the glucina was obtained by distilling off the carbonate of ammonia and collecting on a filter. The carbonate of ammonia was thus saved for future operations. A serious objection to this method is the fact that considerable alumina is always dissolved in the presence of glucina, although alone it is not affected by carbonate of ammonia. I instituted a series of experiments in order to ascertain the degree of concentration of the carbonate of ammonia, and the length of time most favorable for the solution of the glucina.

One gramme of pure glucina was treated with carbonate of ammonia, under the same circumstances of temperature and concentration, for three, seven, eleven, and sixteen days. After ten days, with carbonate of ammonia of 1·080 specific gravity and 15° C., the maximum amount was dissolved. If the solution be kept longer than ten days, a precipitate of carbonate of glucina will begin to form, and at the expiration of sixteen days, fifteen per cent less of the original amount will go into solution. It was found advisable to separate the glucina after the expiration of a week. I observed that it was preferable to precipitate the two earths in the first instance with carbonate of ammonia, as the glucina was then more soluble than if first thrown down by ammonia and afterwards digested in the carbonate. Carbonate of potassa and carbonate of soda dissolve the precipitate of glucina, but with greater difficulty.

3. *By caustic potassa.*—This method was first proposed by C. G. Gmelin.[8] The solution of alumina and glucina in chlorhydric acid is neutralized by a cold solution of caustic potassa until the precipitate disappears; it is then largely diluted with water, and boiled in a platinum capsule for a quarter of an hour. The glucina will be precipitated, and must be carefully washed in hot water to free it of all traces of potassa.

By diluting the potassa with ten parts of water, and dissolving the glucina in chlorhydric acid, and re-precipitating by ammonia, I obtained very pure glucina, but always with loss of mate-

[7] Pogg. Ann., xcii, 91.

[8] Handwörtb. d. Chem., 2te Auf., ii, 1018. Pogg. Ann., xcii, 97.

rial, as a portion of the glucina remained in solution with the alumina, and where iron was present, I found that considerable quantities were dissolved by the potassa, notwithstanding every precaution was observed.

4. *By sulphurous acid.*—This method of Berthier,[9] founded upon the difficult solubility of the basic sulphite of alumina, did not succeed, as variable quantities of the sulphite of glucina were always thrown down with the alumina salt. The sulphite of ammonia was substituted for the sulphurous acid gas, but the result was the same. My observations confirmed the experience of Weeren[10] and Böttinger.[11]

5. *By carbonate of baryta.*—According to H. Rose,[12] carbonate of baryta does not precipitate glucina from cold solutions, while alumina under the same circumstances is precipitated. I found, however, in confirmation of the observations of Weeren,[13] that both glucina and alumina were precipitated.

6. *By hyposulphite of soda.*—Chancel's[14] method for the separation of alumina and iron was applied to the separation of alumina and glucina. Weighed portions of alumina and glucina were dissolved in chlorhydric acid, nearly neutralized by carbonate of soda, largely diluted, and to the cold liquid a slight excess of hyposulphite of soda added, and the whole boiled until fumes of sulphurous acid were no longer observed. It was found that the glucina was precipitated along with the alumina, and the method proved unavailing.

7. *By the decomposition of the nitrates of alumina and glucina.*—According to Deville,[15] if the nitrate of alumina be heated for some time to 200° and 250° C., all the nitric acid will be expelled, and a residue of granular alumina, insoluble in water, will be left in the capsule. Alumina can in this way be separated from BaO, SrO, CaO, KO, NaO, and MgO.

Weighed portions of the nitrates of alumina and glucina were heated together to 200° C., and afterwards treated with water. It was found that the nitrate of glucina was decomposed the same as the nitrate of alumina—affording no method for the separation of the earths.

8. *By acetate of soda.*—Alumina is precipitated from a boiling solution of acetate of soda, the same as the oxyd of iron, a method employed for the separation of alumina and iron from other bases. I found that the behavior of glucina was the same as alumina, and consequently this method was also unavailing.

9. *By fusing with caustic potassa.*—Weeren[16] says, if glucina

[9] Rose, Handb. d. Analyt. Chem., ii, 60. Berz. Jahrb., xiii, 148.
[10] Pogg. Ann., xcii, 99. [11] Ann. Chem. Pharm., li, 397.
[12] Rose, Handb. d. Analyt. Chem., ii, 57, 61.
[13] Pogg. Ann., xcii, 104.
[14] Comp. Rend., xlvi, 987. Ann. Chem. Pharm., cviii, 237.
[15] Ann. Chim. Phys., [3], xxxviii, 9. [16] Pogg. Ann., xcii, 106.

and caustic potassa be fused together, water dissolves out nothing, but if alumina be fused in the same way, it will be rendered soluble in water. This suggested a method for the separation of the two earths. Upon trial it was found that the glucina was equally soluble in water after fusion with caustic potassa.

10. *By formate of ammonia.*—Formate of ammonia precipitates iron, alumina, and glucina, and none of them are soluble in excess. This method was therefore not available.

11. *By the decomposition of sulphates.*—If sulphate of glucina be heated to redness, sulphuric acid and sulphur are driven off, and the pure oxyd remains. The sulphate of manganese is not decomposed by heat in this way. This method may be of application in the analysis of helvine, but can not be used to separate alumina from glucina, as the sulphate of alumina behaves, when heated, in the same manner as the sulphate of glucina.

12. *By the formation of potash-alum.*—One part of finely pulverized beryl was intimately mixed with two parts of carbonate of potassa, and fused in a capacious platinum crucible at an ordinary red heat. After cooling, concentrated sulphuric acid was poured over the mass, care being taken to prevent loss by effervescence, and the whole constantly stirred until it assumed a gelatinous condition. The excess of sulphuric acid was then expelled, and the silica determined as usual. The filtrate from the silica, containing the sulphates of alumina, glucina, iron and potassa, was concentrated by evaporation, and allowed to stand twenty-four hours, and sometimes longer, according to the amount of beryl taken, until a crop of alum crystals had formed. These were collected and washed, and the liquid evaporated until a second crop was gathered. The filtrate from the alum crystals was concentrated and poured into a hot saturated solution of carbonate of ammonia, and allowed to stand several days with frequent agitation. The insoluble portion was collected on a filter and digested a second time in carbonate of ammonia and afterwards in caustic potassa. The portion insoluble in caustic potassa was collected and weighed, and gave the percentage of iron. The filtrate from the iron was examined for alumina and yielded only traces, showing that all of the alumina was separated as alum. The glucina and alumina were determined as oxyds. This method was subjected to repeated trials, and was found to give better results than any other. The average composition of the beryl from Acworth, New Hampshire, determined in this way on a large scale, proved to be as follows: silica 68·84, glucina 13·40, alumina 16·47, sesquioxyd of iron 1·70. Total 100·41.

The consideration of the salts of glucina is reserved for a future communication.

Columbia College, New York, May, 1863.

ART. XIII.—*Remarks on the Luminosity of Meteors as affected by Latent Heat;* by BENJAMIN V. MARSH.

IN the January number of this Journal, in the notice of Mr. Quetelet's work, "*Sur la Physique du Globe,*" objection is made to certain "novel ideas on the constitution of the atmosphere," —apparently adopted by the author mainly for the purpose of explaining the fact that shooting stars and meteorites always disappear before reaching the earth. The notice says, "we observe these meteors at elevations of 140 to 160 miles; they increase in brightness as they approach the earth; they disappear entirely as they approach the lower part of the atmosphere, as if they entered a medium which had not the elements necessary for their continued brilliancy."

The aim of the present paper is to show that the well-established fact thus stated may be fully and satisfactorily explained by other means.

Mr. Birks, in his chapter on the "*Igneous condition of matter,*" says,[1] "There will thus, according to the present theory of the laws of matter, be more truth than has latterly been recognized in the old arrangement of the four elements, which placed a fourth region of fire above the solid, liquid, and gaseous constituents of our globe. In fact, above the region where the air, though greatly rarefied, is still elastic, there must be a still higher stratum where elasticity has wholly ceased, and where the particles of matter, being very widely separated, condense around them the largest amount of ether. All sensible heat, in the collision or oscillation of neighboring atoms of matter, will thus have disappeared; but latent heat, in the quantity of condensed ether or repulsive force ready to be developed on the renewed approach of the atoms, will have reached its maximum, and may be capable of producing the most splendid igneous phenomena, like the northern lights, or tropical thunder storms."

On reading the above, I was so struck with its peculiar adaptation to the explanation of meteoric phenomena, that I was induced to inquire, without any regard to the theoretical views of the author—what, according to the accepted laws of heat, must be the condition of the upper regions of the atmosphere in reference to latent heat?

"If a unit of weight of any gas, allowed to expand freely without change of pressure, is heated from the freezing point one degree, the amount of heat thus absorbed, measured in fractions of the unit, is called 'the specific heat under constant pressure.' If the same gas is heated one degree when so confined

[1] On Matter and Ether, or The Secret Laws of Physical Change; by Thomas Rawson Birks, M.A. Cambridge (England), 1862.

that its volume can not be increased, the amount of heat required to produce the change of temperature is called 'the specific heat under a constant volume.'"—*Silliman's Physics*, p. 459.

The specific heat of air under constant pressure (that of water being unity) has been found to be 0·2377; specific heat of air under constant volume has been found to be 0·1678; difference 0·0699.

"Comparing these results in the case of air, we see that, when air is heated in a situation where it is free to expand, only about $\frac{1}{7}$ of the heat applied is expended in producing elevation of temperature—as in heating a room—while about $\frac{2}{7}$ of the heat is expended in producing expansion of the air, to be given out again as the room cools."—*Silliman's Physics*, p. 451.

Again: "It is a perfectly well known fact that a certain amount of heat is rendered latent in producing the expansion of a given mass of gas, and that, on condensing the gas to its original volume, the same amount of heat is set free."—*Cooke's Chemical Physics*, p. 480.

The absorption of a certain amount of heat, and the rendering of it latent, appears to be admitted as a necessary accompaniment of the act of expansion, as such, and essential to its accomplishment—whether the expansion be produced by the removal of pressure,[2] or by the application of heat, or by both combined. The amount absorbed must therefore depend solely upon the extent of the expansion—and air of any given density must always contain the same amount of latent heat, no matter what may be its past history or its present condition as to temperature or pressure.

Now it has been ascertained by Regnault and others, that when air is heated in a situation where it is free to expand without change of pressure, equal additions of heat make equal additions of volume—and that this holds good at all temperatures and at all pressures—also that the rate of expansion is such that air at the freezing point expands $\frac{1}{491}$ part of its bulk for every added degree of heat on Fahrenheit's scale. That is

491 cubic inches of air at 32° become
492 " " " 33 "
493 " " " 34 " &c.

[2] A striking instance of the effect of the removal of pressure is afforded on a vast scale at the fountain of Hiero, at the mines of Chemnitz in Hungary. "A part of the machinery for working these mines is a perpendicular column of water 260 feet high, which presses on a quantity of air enclosed in a tight reservoir. The air is consequently condensed to an enormous degree by this height of water, which is equal to 8 or 9 atmospheres; and when a pipe communicating with the reservoir of condensed air is suddenly opened, it rushes out with extreme velocity, instantly expands, and in so doing absorbs so much caloric as to precipitate the moisture it contains in a shower of very white compact snow, or rather hail, which may be readily gathered in a hat held in the blast."—*Silliman's Chemistry*, 1830, vol. i, p. 121.

Whence it appears that

1 vol. of air at 32°, by having its temperature raised 491° becomes 2 vols.
1 " " " " " " " 982 " 3 "
1 " " " " " " " 1473 " 4 "

&c.,—the increase being 1 volume for each 491°.

But it has already been shown that, of the heat employed in this process, about $\frac{2}{7}$ (more exactly $\frac{699}{3777}$) is absorbed by the air and rendered latent. Hence, of each 491° expended as above, $\frac{699}{3777}$, or about 144°, are rendered latent. It therefore follows that

Vol. of air.										Latent heat.
1	at 32° by having its temp. raised 491°					becomes	2	vols. and contains		144°
1	"	"	"	"	"	982	"	3	" "	288
1	"	"	"	"	"	1473	"	4	" "	432

and so on indefinitely.

Now, inasmuch as it is known that at the height of 3·43 miles the volume of a given weight of air is twice what it is at the earth's surface, and that as we ascend the number of volumes is doubled for each addition of 3·43 miles to the height, the above considerations enable us to calculate the amount of latent heat in any given weight or bulk of air at any given height within the limits of the atmosphere.

Height in miles. n being — number terms of this series, a being —3·43. na.	Number of volumes corresponding to 1 volume at the surface of the earth. 2^n.	Number of degrees of latent heat. $144\,(2^n - 1)$.	Latent heat in 1 volume to nearest degree. $144\left(\frac{2^n-1}{2^n}\right)$	Number of grains of air in cylinder 1 mile long and 1 foot in diameter. Weight at surface of the earth —2342847 grains —334·69 pounds avoirdupois.
3·43	2	144	72	1171424
6·86	4	432	108	585712
10·29	8	1008	126	292856
13·72	16	2160	135	146428
17·15	32	4464	139	73214
20·58	64	9072	142	36607
24·01	128	18288	143	18303
27·44	256	36720	143	9152
30·87	512	73584	144	4576
34·30	1024	147312	144	2288
37·73	2048	294768	144	1144
41·16	4096	589680	144	572
44·59	8192	1179504	144	286
48·02	16384	2359152	144	143
51·45	32768	4718448	144	72
54·88	65536	9437040	144	36
58·31	131072	18874224	144	18
61·74	262144	37748592	144	9
65·17	524288	75497328	144	4
68·60	1048576	150994800	144	2
102·90	1073741824	154618822512	144	$3\overline{)2}$
137.20	1099511627776	158329674899600	144	$324\overline{)288}$
171·50	1125899906842624	162129586585337712	144	$3888\overline{)10812}$
205·80	1152921504606846976	166020696663385964400	144	$34973581\overline{)33333}$

The above table shows results thus obtained, together with some other facts bearing upon the subject.

The most important as well as the most striking fact shown by this table, is that the quantity of latent heat in a given bulk of air is sensibly constant for all heights exceeding 30 miles. Below that point it decreases rapidly as we descend, being at 3½ miles only one-half of what it is at 30 miles.

For convenience of illustration, let us assume as our unit of measure a cylinder 1 mile long and 1 foot in diameter—this being the space traversed by a globular meteor 1 foot in diameter in going 1 mile. Such a cylinder will contain, at the surface of the earth, 335 pounds (2342847 grains) of air.

At the height of 3·43 miles it will contain 167 pounds, which, when condensed to the density of air at the surface, will evolve enough heat to raise the temperature of the original weight—say 335 pounds—72°.

At 34·30 miles it will contain ½ pound of air, which, condensed as before, will evolve heat sufficient to raise 335 pounds 144°.

At 68·60 miles—the weight of air is only 2 grains but its condensation will raise 335 pounds 144°—and *generally, the same bulk of air is capable of effecting the same result at any greater height, even to the extreme limits of the atmosphere.*

Now let us suppose a meteoric stone one foot in diameter (weighing say two hundred pounds) to enter the atmosphere with a velocity of ten miles per second.

In every mile that it travels, it meets with and condenses before it a bulk of air equal to our assumed unit of measure, which, compressed to the density of air at the surface of the earth, will give out heat enough to raise 335 pounds of air 144°. In one second it passes through ten units, and the heat evolved will raise 335 pounds 1440°, or the weight of the stone—two hundred pounds—2412°, being more than sufficient to bring the whole mass to an incandescent state.

But this heat is developed, *not* in the stone weighing 200 lbs., but in a body of air, the total weight of which is at most only a few grains. The intensity of the heat in this small mass must therefore be proportionally greater. The table shows that at the height of only 55 miles the heat is sufficient to raise the temperature of the whole mass of air encountered, more than nine million degrees; and at greater heights the intensity will increase in a geometric ratio, so that at 137 miles only it becomes one hundred and fifty-eight millions of millions.

It thus appears that we have the means of accounting for a brilliancy of any imaginable intensity—*the greatest splendor being, not in the meteor, but in the air which surrounds it.*

Those particles of air which are in *immediate contact* with the stone (and those only) will of course part with a considerable

portion of their heat, which, as time is not afforded for it to penetrate the mass, must be expended in burning off or vaporizing the surface layer of the stone. The greatest elevation of temperature must evidently take place in the remaining portions of the air, which, retaining nearly the whole of the heat developed in them, will reach a state of the most brilliant incandescence, the splendor of which will be vastly increased by the presence of the stony particles thrown off from the meteor.

For convenience I have assumed that the air is in all cases compressed to the density of that at the surface of the earth. Whilst this is doubtless sufficiently correct for the purpose of illustrating the nature of the phenomena, it can of course lay no claim to accuracy. The density attained must vary with the height, velocity, &c. Again, the sudden elevation of temperature must generate an enormous elasticity which will tend to drive the incandescent particles outward from the axis of the meteor's path and thus limit the degree of condensation.

In this way the most intensely brilliant particles must be directly in front of the meteor, streaming outward in all directions. As these are left behind by the meteor in its flight they must form a cylinder of fire; but the expansion which immediately ensues, promptly cools it off, and as this cooling process must begin at the surface of the cylinder and can only reach the axis at some distance in the rear of the meteor, it will evidently convert the cylinder into a luminous cone moving base-foremost —far the greatest brilliancy being in the base itself. This is precisely the form actually observed in the great daylight meteors of 1859 and 1860 and in some others of the same class.

The conclusion to which these considerations lead, is that the upper regions of the atmosphere, even to its utmost limit, are grand reservoirs of latent heat[3] most admirably adapted for the

[3] Sir John Herschell in his "*Outlines of Astronomy*," p. 617, says, "The heat which they possess when fallen, the igneous phenomena which accompany them, their explosion on arriving within the denser regions of our atmosphere, &c., are all sufficiently accounted for on physical principles, by the condensation of the air before them in consequence of their enormous velocity, and by the relations of air in a highly attenuated state to heat," and he refers to the *Edinburgh Review*, Jan., 1848, p. 195. The passage in the Review is as follows:

"Arriving with planetary velocity at the confines of our atmosphere, where the air is many thousand, perhaps million times rarer than at the surface of the earth, such a body would carry before it the air on which it immediately impinged, compressing it to an enormous *relative* extent against its own surface, before the *absolute* compression could reach such a point as to determine its lateral escape. Now it has been shown by Poisson (*Ann. de Chim.*, xxiii, 341) that the latent heat of a given weight of air is greater, the lower the pressure under which it exists. A given quantity (by weight) of air, therefore, at those elevations contains more latent heat than the same quantity at the earth's surface. When condensed, therefore, it will give out *more* heat than would be elicited by the same extent of *relative* condensation from air of ordinary density, which we know to be capable of producing ignition, even under very moderate degrees of sudden compression. A source of sudden and transient heat of almost any conceivable intensity, is thus provided in immediate contact with the surface of the stone, which it would fuse and partially

protection of the earth from collision with bodies approaching it with planetary velocity from without. The intruder is instantly surrounded with a fiery envelope heated to the greatest conceivable intensity, its surface is burned off or dissipated into vapor, the sudden expansion of the stratum immediately beneath the burning surface tears the body into fragments, each of which, retaining its planetary velocity, is instantly surrounded by a similar envelope, which produces like effects; and so on, until in most cases the whole is burnt up or vaporized.

Of the vast number of meteors seen, and which may fairly be presumed to embrace great variety of material, but very few are known to reach the earth, and these few are invariably found to be composed of the most incombustible substances—flinty stones or masses of iron. Such bodies may penetrate the whole depth of the atmosphere with only a partial loss of substance, whilst those of a more combustible nature may be totally destroyed during the flight of a few miles.

vaporize, while the sudden and violent expansion of the parts immediately beneath the fused film must necessarily cause decrepitation and disruption of fragments. In short, there is no part of the phenomenon which this explanation does not reach. Mere friction against the atmosphere, as suggested by Poisson, seems quite insufficient to produce incandescence."

Although no numerical results are here given, it might be supposed that this article anticipates those given above, but such does not appear to be the fact.

The conclusion to which the mathematical investigations of Poisson led him are thus announced (*Ann. de Chim.*, 1825, xxiii, 341). "Whence we conclude in general,

$$c = (0{\cdot}2669)\left(\frac{P}{p}\right)^{1-\frac{1}{k}}$$

and the value of c' can be deduced from that of c by dividing it by k. As this quantity k is greater than unity, we see that the *specific* heat of a gramme of air, and generally of any gas whatever, will increase when the elastic force, p, becomes less."

In the above, c represents the specific heat under constant pressure.
c' " " " " volume.
p " the pressure.
P " " with barometer at 29·92 in.

and $k = \frac{c}{c'}$ and assumed $= 1{\cdot}3750$.

Poisson treats only of "specific heat," and makes no mention of "latent heat" in any part of the article ('Sur la Chaleur des gaz et des vapeurs.')

But even overlooking this misquotation, and assuming, as the writer seems to have done, that, for the object in view, "specific heat" and "latent heat" might be treated as convertible terms, the ratio of increase with the increase of height is altogether too trifling to serve as the basis of any explanation of the phenomena in question. At the height of 41 miles, the specific heat, according to this formula, would be only ten times that at the surface of the earth; whereas the latent heat at that height as above stated is 589680 degrees.

But, furthermore, Poisson's result was a mere theoretical deduction, which has been proved to be altogether erroneous by the experiments of Regnault, who has shown that the *specific heat is the same for all pressures;* so that the explanation as it stands in the Review appears to be entirely without foundation.

If, as above maintained, the observed splendor is not due to the temperature of the meteoric bodies themselves, but to that of mere envelopes of air, brought to the most intense degree of incandescence by the development of their latent heat, it is evident, that, inasmuch as this heat is nearly constant for all considerable heights, the most splendid results must be developed in the rarest portions of the atmosphere, because there the mass of the air to be acted upon by this fixed amount of heat is least; and that as we descend, a point may be reached where the mass is so great that the intensity will fall short of that required to produce incandescence, and all luminosity must instantly cease. The meteor will then have "entered a medium which has not the elements necessary to its continued brilliancy."

The table shows that at the height of 10¼ miles, with the assumed degree of condensation, the intensity will not exceed one thousand degrees, even without making any allowance for the portion of heat which must always be absorbed by the meteor itself. Luminosity must therefore cease above this limit, and the meteor must perform the remainder of its journey to the earth as a dark body, unless the velocity be such as to produce a much greater condensation. The daylight meteor of November 15, 1859, owing to its amazing velocity, passed this limit, disappearing at the height of only six or eight miles without any perceptible diminution of velocity, but this is believed to be a rare instance.

Whilst the luminous track of those meteors which have their paths directed downward is always cut short before reaching the earth, there are instances of very extended flights, in meteors moving more nearly horizontally. That of July 20th, 1860, was seen to traverse the atmosphere more than a thousand miles, and finally disappeared in the distance over the Atlantic apparently without having become extinct. But this meteor had at no time an elevation less than forty miles, and therefore did not leave the medium which favored its continued brilliancy.

The preceding views may be thought to imply that all meteors should be seen the instant they enter the atmosphere, and consequently at a uniform elevation. But it must be remembered that the extremely cold surface of the meteor (having the temperature of interplanetary space, say 100° below zero) must first be heated; and the distance passed through before this is accomplished must depend upon the size and conducting power, but more especially upon the velocity of the body, and the results may therefore differ widely.

A meteoric body of great conducting power and very moderate velocity might upon first entering the atmosphere absorb so large a portion of the whole heat developed, as to prevent the development of any luminosity until a very considerable depth

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of air had been traversed. On the other hand, we have in the great daylight meteor of 1859 an example of the effects of the most extreme velocity—probably between fifty and a hundred miles per second. This body became visible at a probable height of near two hundred miles, and exhibited a brilliancy almost if not quite equal to that of the sun, being a conspicuous object to persons who were more than two hundred miles from the nearest point in its path, and maintained its luminosity until within a few miles of the earth.

Philadelphia, May 23, 1863.

Art. XIV.—*Proceedings of Learned Societies.—Foreign.*

I. Royal Institution of Great Britain.—Friday, Jan. 23, 1863.

1. *On Radiation through the Earth's Atmosphere;* by John Tyndall, Esq., F.R.S., Professor of Nat. Phil., Roy. Inst.—Nobody ever obtained the idea of a line from Euclid's definition that it is length without breadth. The idea is obtained from a real physical line drawn by a pen or pencil, and therefore possessing width; the idea being afterwards brought, by a process of abstraction, more nearly into accordance with the conditions of the definition. So also with regard to physical phenomena; we must help ourselves to a conception of the invisible by means of proper images derived from the visible, afterwards purifying our conceptions to the needful extent. Definiteness of conceptions, even though at some expense to delicacy, is of the greatest utility in dealing with physical phenomena. Indeed it may be questioned whether a mind trained in physical research can at all enjoy peace, without having made clear to itself some possible way of conceiving of those operations which lie beyond the boundaries of sense, and in which sensible phenomena originate.

When we speak of radiation through the atmosphere, we ought to be able to affix definite physical ideas, to both the term atmosphere and the term radiation. It is well known that our atmosphere is mainly composed of the two elements, oxygen and nitrogen. These elementary atoms may be figured as small spheres scattered thickly in the space which immediately surrounds the earth. They constitute about 99½ per cent of the atmosphere. Mixed with these atoms we have others of a totally different character; we have the molecules, or atomic groups, of carbonic acid, of ammonia, and of aqueous vapor. In these substances diverse atoms have coalesced to form little systems of atoms. The molecule of aqueous vapor, for example, consists of two atoms of hydrogen united to one of oxygen; and they mingle as little triads among the monads of oxygen and nitrogen, which constitute the great mass of the atmosphere.

These atoms and molecules are separate; but in what sense? They are separate from each other in the sense in which the individual fishes of a shoal are separate. The shoal of fish is embraced by a common medium, which connects the different members of the shoal, and renders

intercommunication between them possible. A medium also embraces our atoms; within our atmosphere exists a second, and a finer, atmosphere, in which the atoms of oxygen and nitrogen hang like suspended grains. This finer atmosphere unites not only atom with atom, but star with star; and the light of all suns, and of all stars, is in reality a kind of music propagated through this interstellar air. This image must be clearly seized, and then we have to advance a step. We must not only figure our atoms suspended in this medium, but we must figure them vibrating in it. In this motion of the atoms consists what we call their heat. "What is heat in us," as Locke has perfectly expressed it, "is in the body heated nothing but motion." Well, we must figure this motion communicated to the medium in which the atoms swing, and sent in ripples through it with inconceivable velocity to the bounds of space. Motion in this form, unconnected with ordinary matter, but speeding through the interstellar medium, receives the name of Radiant Heat; and, if competent to excite the nerves of vision, we call it Light.

Aqueous vapor was defined to be an invisible gas. Vapor was permitted to issue horizontally with considerable force from a tube connected with a small boiler. The track of the cloud of condensed steam was vividly illuminated by the electric light. What was seen, however, was not vapor, but vapor condensed to water. Beyond the visible end of the jet the cloud resolved itself into true vapor. A lamp was placed under the jet at various points; the cloud was cut sharply off at that point, and when the flame was placed near the efflux orifice the cloud entirely disappeared. The heat of the lamp completely prevented precipitation.

This same vapor was condensed and congealed on the surface of a vessel containing a freezing mixture, from which it was scraped in quantities sufficient to form a small snowball. The beam of the electric lamp, moreover, was sent through a large receiver placed on an air-pump. A single stroke of the pump caused the precipitation of the aqueous vapor within, which became beautifully illuminated by the beam; while, upon a screen behind, a richly-colored halo, due to diffraction by the little cloud within the receiver, flashed forth.

The waves of heat speed from our earth through our atmosphere towards space. These waves dash in their passage against the atoms of oxygen and nitrogen, and against the molecules of aqueous vapor. Thinly scattered as these latter are, we might naturally think meanly of them as barriers to the waves of heat. We might imagine that the wide spaces between the vapor molecules would be an open door for the passage of the undulations; and that if those waves were at all intercepted, it would be by the substances which form 99½ per cent of the whole atmosphere. Three or four years ago, however, it was found by the speaker that this small modicum of aqueous vapor intercepted fifteen times the quantity of heat stopped by the whole of the air in which it was diffused. It was afterwards found that the dry air then experimented with was not perfectly pure, and that the purer the air became the more it approached the character of a vacuum, and the greater, by comparison, became the action of the aqueous vapor. The vapor was found to act with 30, 40, 50, 60, 70 times the energy of the air in which it was diffused; and no doubt was entertained that the aqueous vapor of the

air which filled the Royal Institution theatre, during the delivery of the discourse, absorbed 90 or 100 times the quantity of radiant heat which was absorbed by the main body of the air of the room.

Looking at the single atoms, for every 200 of oxygen and nitrogen there is about 1 of aqueous vapor. This 1, then, is 80 times more powerful than the 200; and hence, comparing a single atom of oxygen or nitrogen with a single atom of aqueous vapor, we may infer that the action of the latter is 16,000 times that of the former. This was a very astonishing result, and it naturally excited opposition, based on the philosophic reluctance to accept a result so grave in consequences before testing it to the uttermost. From such opposition, a discovery, if it be worth the name, emerges with its fibre strengthened; as the human character gathers force from the healthy antagonisms of active life. It was urged, that the result was on the face of it improbable; that there were, moreover, many ways of accounting for it, without ascribing so enormous a comparative action to aqueous vapor. For example, the cylinder which contained the air, in which these experiments were made, was stopped at its ends by plates of rocksalt, on account of their transparency to radiant heat. Rocksalt is hygroscopic; it attracts the moisture of the atmosphere. Thus, a layer of brine readily forms on the surface of a plate of rocksalt; and it is well known that brine is very impervous to the rays of heat. Illuminating a polished plate of salt by the electric lamp, and casting, by means of a lens, a magnified image of the plate upon a screen, the speaker breathed through a tube for a moment on the salt; brilliant colors of thin plates (soap-bubble colors) flashed forth immediately upon the screen—these being caused by the film of moisture which overspread the salt. Such a film, it was contended, is formed when undried air is sent into the cylinder; it was, therefore, the absorption of a layer of brine which was measured, instead of the absoption of aqueous vapor.

This objection was met in two ways. First, by showing that the plates of salt when subjected to the strictest examination show no trace of a film of moisture. Secondly, by abolishing the plates of salt altogether, and obtaining the same results in a cylinder open at both ends.

It was next surmised, that the effect was due to the impurity of the London air, and the suspended carbon particles were pointed to as the cause of the opacity to radiant heat. This objection was met by bringing air from Hyde Park, Hampstead Heath, Primrose Hill, Epsom Downs, a field near Newport in the Isle of Wight, St. Catharine's Down, and the sea-beach near Black Gang Chine. The aqueous vapor of the air from these localities intercepted at least seventy times the amount of radiant heat absorbed by the air in which the vapor was diffused. Experiments made with smoky air proved that the suspended smoke of the atmosphere of West London, even when an east wind pours over it the smoke of the city, exerts only a fraction of the destructive powers exercised by the transparent and impalpable aqueous vapor diffused in the air.

The cylinder which contained the air through which the calorific rays passed was polished within, and the rays which struck the interior surface were reflected from it to the thermo-electric pile which measured the radiation. The following objection was raised:—You permit moist

air to enter your cylinder; a portion of this moisture is condensed as a liquid film upon the interior surface of your tube; its reflective power is thereby diminished; less heat therefore reaches the pile, and you incorrectly ascribe to the absorption of aqueous vapor an effect which is really due to diminished reflection of the interior surface of your cylinder.

But why should the aqueous vapor so condense? The tube within is warmer than the air without, and against its inner surface the rays of heat are impinging. There can be no tendency to condensation under such circumstances. Further, let five inches of undried air be sent into the tube—that is, one-sixth of the amount which it can contain. These five inches produce their proportionate absorption. The driest day, on the driest portion of the earth's surface, would make no approach to the dryness of our cylinder when it contains only five inches of air. Make it 10, 15, 20, 25, 30 inches: you obtain an absorption exactly proportional to the quantity of vapor present. It is next to a physical impossibility that this could be the case if the effect were due to condensation. But lest a doubt should linger in the mind, not only were the plates of rocksalt abolished, but the cylinder itself was dispensed with. Humid air was displaced by dry, and dry air by humid in the free atmosphere; the absorption of the aqueous vapor was here manifest, as in all the other cases.

No doubt, therefore, can exist of the extraordinary opacity of this substance to the rays of obscure heat; and particularly such rays as are emitted by the earth after it has been warmed by the sun. It is perfectly certain that more than ten per cent of the terrestrial radiation from the soil of England is stopped within ten feet of the surface of the soil. This one fact is sufficient to show the immense influence which this newly-discovered property of aqueous vapors must exert on the phenomena of meteorology.

This aqueous vapor is a blanket more necessary to the vegetable life of England than clothing is to man. Remove for a single summer-night the aqueous vapor from the air which overspreads this country, and you would assuredly destroy every plant capable of being destroyed by a freezing temperature. The warmth of our fields and gardens would pour itself unrequited into space, and the sun would rise upon an island held fast in the iron grip of frost. The aqueous vapor constitutes a local dam, by which the temperature at the earth's surface is deepened: the dam, however, finally overflows, and we give to space all that we receive from the sun.

The sun raises the vapors of the equatorial ocean; they rise, but for a time a vapor screen spreads above and around them. But the higher they rise, the more they come into the presence of pure space, and when, by their levity, they have penetrated the vapor screen, which lies close to the earth's surface, what must occur?

It has been said that, compared atom for atom, the absorption of an atom of aqueous vapor is 16,000 times that of air. Now the power to absorb and the power to radiate are perfectly reciprocal and proportional. The atom of aqueous vapor will therefore radiate with 16,000 times the energy of an atom of air. Imagine then this power-

ful radiant in the presence of space, and with no screen above it to check its radiation. Into space it pours its heat, chills itself, condenses, and the tropical torrents are the consequence. The expansion of the air, no doubt, also refrigerates it; but in accounting for those deluges, the chilling of the vapor by its own radiation must play a most important part. The rain quits the ocean as vapor; it returns to it as water. How are the vast stores of heat set free by the change from the vaporous to the liquid condition disposed of? Doubtless in great part they are wasted by radiation into space. Similar remarks apply to the cumuli of our latitudes. The warmed air, charged with vapor, rises in columns, so as to penetrate the vapor screen which hugs the earth; in the presence of space, the head of each pillar wastes it heat by radiation, condenses to a cumulus, which constitutes the visible capital of an invisible column of saturated air.

Numberless other meteorological phenomena receive their solution, by reference to the radiant and absorbent properties of aqueous vapor. It is the absence of this screen, and the consequent copious waste of heat, that causes mountains to be so much chilled when the sun is withdrawn. Its absence in Central Asia renders the winter there almost unendurable; in Sahara the dryness of the air is sometimes such, that though during the day "the soil is fire and the wind is flame," the chill at night is painful to bear. In Australia, also, the thermometric range is enormous, on account of the absence of this qualifying agent. A clear day, and a dry day, moreover, are very different things. The atmosphere may possess great visual clearness, while it is charged with aqueous vapor, and on such occasions great chilling cannot occur by terrestrial radiation. Sir John Leslie and others have been perplexed by the varying indications of their instruments on days equally bright—but all these anomalies are completely accounted for by reference to this newly-discovered property of transparent aqueous vapor. Its presence would check the earth's loss; its absence, without sensibly altering the transparency of the air, would open wide a door for the escape of the earth's heat into infinitude.

II. Proceedings of the Royal Society, Vol. xii, No. 51.

2. *On the Photographic Transparency of various Bodies, and on the Photographic Effects of Metallic and other Spectra obtained by means of the Electric Spark;* by Prof. W. Allen Miller.—In this paper the author pursues an inquiry the commencement of which was communicated to the Chemical Section of the British Association last year. Owing to the employment of a prism of bisulphid of carbon, he was then led to believe that the photographic effects of the electric spectra produced by the different metals were in a great degree similar, if not identical. Subsequent investigations have, however, shown him that the absorbent effects of the bisulphid upon the chemical rays are so great, that the conclusions then drawn from observations made by this refracting medium require very considerable modification. Notwithstanding the great length of the chemical spectra obtained by the aid of the bisulphid, not more than *one-sixth* or *one-seventh* of the true extent of the spectrum produced by the electric spark between various metals is procured, as may be shown

by comparing the spectrum with one of the same metal furnished by the use of a lens and prism of rock-crystal.

Rock-crystal, however, possesses but a comparatively small refractive and dispersive power, whilst it almost always affords some trace of double refraction in one portion or other of the spectrum procured by its means.

In searching for some singly refracting medium which should possess sufficient refractive and dispersive power to enable it to be used advantageously in the construction of lenses and prisms suitable for this inquiry, the author was led to examine the photographic absorption of a variety of colorless substances which appeared perfectly transparent to the luminous rays. The experiments detailed in the first portion of the present paper refer to this absorbent action of various media upon the chemical rays of the spectrum; whilst the second portion of the paper is devoted to a description of the electric spectra of some of the more important elementary bodies, and the effect of varying the gaseous media in which the sparks producing these spectra are made to originate.

(1.) *The Photographic Transparency of Bodies.*—In the experiments upon the absorbent action of the different media, the source of light employed was the electric spark obtained between two metallic wires (generally of fine silver), connected with the terminals of the secondary wires of a ten-inch induction-coil. The light, after passing through a narrow vertical slit, either before or after traversing a stratum of the material the chemical transparency or *diactinic* quality of which was to be tested, was allowed to fall upon a quartz prism placed at the angle of minimum deviation for the mean of the refracted rays. Immediately behind this was a lens of rock-crystal, and behind this, at a suitable distance, the spectrum was received upon a collodion-film coated with iodid of silver; this supported in the frame of a camera, and after an exposure, generally lasting for five minutes, the image was developed by means of pyrogallic acid, and fixed with cyanid of potassium.

The general results of these experiments were as follows:—

1. Colorless bodies, which are equally transparent to the visible rays, vary greatly in permeability to the chemical rays.

2. Bodies which are photographically transparent, in the solid form, preserve their transparency in the liquid and in the gaseous states.

3. Colorless transparent solids, which exert a considerable photographic absorption, preserve their absorptive action with greater or less intensity both in the liquid and the gaseous states.

Whether the compound is liquefied by heat or dissolved in water, these conclusions respecting liquids are equally true. The perfect permeability of water to the chemical rays, conjoined with the circumstance that in no instance does the process of solution seem to interfere with the special action upon the incident rays of the substance dissolved, renders it practicable to submit to this test a great number of bodies which it would otherwise be impossible to subject to this species of experiment on account of the extreme difficulty of obtaining them in crystals of sufficient size and limpidity.

Glass vessels cannot be employed to contain the liquids during the trial. Flint-glass, crown, hard white Bohemian, plate-glass, window-sheet, and Faraday's optical glass, all, even in thin layers, shorten the spectrum by

from three-fifths to four-fifths or even more of its length. Mica produces a similar effect. Indeed, the only substance which the author found could be employed with advantage is rock-crystal cut into thin slices and polished. The value of this material in researches upon the more refrangible end of the spectrum was pointed out by Prof. Stokes and Ed. Becquerel several years ago. In order to hold the liquids for experiment, a small trough was prepared by cutting a notch in a thick plate of plate-glass, the sides being completed by means of thin plates of quartz, which were pressed against the ground surfaces of the plate-glass by the aid of elastic bands of caoutchouc; a stratum of liquid of 0·75 inch in depth was thus obtained for each experiment.

The substances which, after atmospheric air and certain other gases, are most perfectly diactinic, are rock-crystal, ice, as well as pure water, and white fluor-spar. Rock-salt is scarcely inferior to them, if at all. Then follow various sulphates, including those of baryta, and the hydrated sulphates of lime and magnesia, as well as those of the alkalies. The carbonates of the alkalies and alkaline earths, as also the phosphates, arseniates, and borates, are likewise tolerably transparent, though saturated solutions of phosphoric and arsenic acids exerted considerable absorbent power; so also did those of the alkalies, potash, and soda, possibly from the presence of a trace of some foreign coloring matter, as those liquids had an extremely faint greenish tinge.

The soluble fluorids, as well as the chlorids and bromids of the metals of the alkalies and alkaline earths, are freely diactinic, but the iodids are much less so, and exhibit certain peculiarities. All the organic acids and their salts which were tried by the author exerted a marked absorbent action upon the more refrangible rays. Amongst those subjected to experiment were the oxalates, tartrates, acetates, and citrates, those mentioned first in order having the greatest absorbent action. It is, however, much more difficult to obtain organic compounds in a state of purity sufficient to furnish trustworthy results, than is the case with the salts of the inorganic acids. The author, therefore, expresses himself with more reserve upon some of these organic bodies, particularly the acetates, than in other cases. The different varieties of sugar are freely diactinic.

Amongst the salts of inorganic acids, the nitrates are the most remarkable for their power of arresting the chemical rays. A solution of each of these salts, in all the instances tried, cut off all the more refrangible rays, and reduced the spectrum to less than a sixth of its ordinary length. The chlorates, however, do not participate in this absorptive power to nearly the same extent.

Although the sulphates, as a class, are largely diactinic, the sulphites are much less so; and the hyposulphites cut off about three-fourths of the length of the spectrum, leaving only the less refrangible portion.

Of eighteen different liquids tried by the author, two only can be regarded as tolerably diactinic, viz: water, which is eminently so, and absolute alcohol, which, however, exhibits a considerable falling off. The liquids which follow are mentioned in the order of their chemical transparency, those most transparent being mentioned first:—Dutch liquid, chloroform, ether; then benzole and distilled glycerin, which differ but

little; then fusel oil, wood-spirit, and oxalic ether, which are also nearly alike; acetic acid, oil of turpentine, glycol, carbolic acid, liquid paraffin, boiling at 360° F., and bisulphid of carbon. Finally, terchlorid and oxychlorid of phosphorus, although perfectly colorless and limpid, arrest all the chemical rays.

The experiments upon aeriform bodies yielded important results; they show but little coincidence with those of Tyndall on the absorptive power of the gases for radiant heat. These experiments were made by interposing in the track of the ray between the vertical slit and the quartz prism, a brass tube two feet long, closed at each end air-tight by means of a plate of quartz. Each gas or vapor in succession was introduced into the tube, and the results compared with those produced by causing the rays to traverse the tube when filled with atmospheric air.

Amongst the colorless gases, oxygen, hydrogen, nitrogen, carbonic acid, and carbonic oxyd exhibit no absorptive power.

Olefiant gas, protoxyd of nitrogen, cyanogen, and chlorhydric acid exert a slight but perceptible absorbent effect. But in the case of coal-gas the absorptive action is extremely marked, the more refrangible half of the spectrum being cut off by it abruptly. The absorption exerted by sulphurous acid is still more powerful and as sharply defined; sulphuretted hydrogen and the vapor of bisulphid of carbon exhibit a still more decided absorbent action; the effect of the terchlorid and oxychlorid of phosphorus is not less marked. This absorbent action of these different compounds of sulphur and phosphorus is very striking.

Coal-gas appears to owe its remarkable power of arresting the chemical rays to the presence of the vapor of benzol and other heavy hydrocarbons; since the vapor of benzol at 65°, diffused to saturation through a column of atmospheric air two feet long, exerts a still more powerful absorbent effect than coal-gas.

On the other hand, the effect of a similar arrangement, in which the vapor of ether, of chloroform, and of oil of turpentine was substituted for that of benzol, gave effects which, though perceptible, were much less marked. An arbitrary scale is laid down, by which a comparative estimate of the absorptive power of each compound, whether solid, liquid, or gaseous, may be effected with tolerable accuracy.

With a view of facilitating the production of a spectrum on a flat field, at a uniform distance at all points from the prism, the author instituted a series of experiments, in which a small metallic speculum was substituted for the lens of rock-crystal; but the loss of chemical power in the reflected rays was so considerable, and this loss occurred so unequally at different points, that the method was abandoned. The results of the photographic action of light reflected at an angle of 45° from the polished surface of several of the principal metals is given. The reflection from gold, although not very intense, was found to be more uniform in quality than that from any other metal that was tried. Burnished lead also gave very good results. The reflection from silver is singularly deficient in some portions of the less refrangible rays, although in most other parts the reflection is tolerably perfect, except for rays of extremely high refrangibility.

3. *The Electric Spectra of the Metals.*—The author proceeds then to detail his experiments upon the spectra obtained by causing the sparks of the secondary current from the induction-coil to pass between electrodes composed of various elementary substances, and he gives photographs of the impressions obtained from collodion negatives of a considerable number of different elementary bodies. The spectra were procured by arranging a quartz-train in the manner already described. Among the elements so examined are the following:—

Platinum,	Arsenic,	Copper,
Palladium,	Tellurium,	Aluminum,
Gold,	Tungsten,	Cadmium,
Silver,	Molybdenum,	Zinc,
Mercury,	Chromium,	Magnesium,
Lead,	Manganese,	Sodium,
Tin,	Iron,	Potassium,
Bismuth,	Cobalt,	Graphite, and
Antimony,	Nickel,	Gas-coke.

The commencement of each spectrum in its less refrangible portion is similar in nearly all cases; and, as it is this portion only which is transmissible through bisulphid of carbon, this circumstance explains the similarity of all the spectra procured by the author from different metals in his earlier experiments, already laid before the British Association. In the more refrangible parts of the spectrum, great and characteristic differences between the results obtained with the different metals are at once manifest. In some cases, as in those of copper and nickel, the action is greatly prolonged in the more refrangible extremity, whilst the intense and highly characteristic spectrum of magnesium is much shorter.

In many cases, metals which are allied in chemical properties exhibit a certain similarity in their spectra. This occurs, for example, with the magnetic metals, iron, cobalt, and nickel, and with the group embracing bismuth, antimony, and arsenic. The more volatile metals exhibit generally the most strongly marked lines. Cadmium, for instance, gives two intense groups. Zinc, two very strong lines near the less refrangible extremity, three near the middle, and four nearly equidistant lines towards the termination of the more refrangible portion; whilst in the spectrum of magnesium the chemical action is almost suddenly terminated near the middle by a triple group of very broad and strong lines.

It will be observed, on examining the photographs of these spectra of the various metals, that the impressions, particularly in the more refrangible portions, consist of a double row of dots, running parallel with the length of the spectrum, and forming the terminations of lines rather than lines themselves, as though the intense ignition of the detached particles of metal, necessary to furnish rays capable of exciting chemical action, had ceased before the transfer of these particles to the opposite electrode had been completed.

If each electrode be composed of a different metal, the spectrum of each metal is impressed separately upon the plate, as is evident on examining the photographs.

When alloys are employed as electrodes, the spectrum exhibited is that

due to both the metals; but if the metals made use of are approximatively pure, the spectrum is hardly to be distinguished from that of the pure metal. In the case when alloys are used as electrodes, it is not always the more volatile metal which impresses its spectrum most strongly. A specimen of brass, for example, containing 38 per cent of zinc, gave a spectrum which could not be distinguished from that of pure copper, though an alloy of three parts of gold and one of silver gave a spectrum in which the lines due to silver predominated.

The author then proceeds to describe a number of experiments upon the transmission of sparks between electrodes of different metals in a current of several different gases. The apparatus employed consisted of a glass tube; into the side an aperture was drilled, which could be closed by a plate of quartz; the ends of the tubes were closed by ground brass plates, each supporting a pair of brass forceps, into which the electrodes were fitted; through the axis of the tube a current of each gas was transmitted at the ordinary atmospheric pressure.

Among the gases thus tried were hydrogen, protoxyd of nitrogen, carbonic acid, carbonic oxyd, olefiant gas, marsh-gas, cyanogen, sulphuretted hydrogen, sulphurous acid, nitrogen, and oxygen. The spectrum obtained from the same metal varied considerably in these different media. In hydrogen, the intensity of the spectrum was greatly reduced, and the more refrangible rays were wanting, but no new rays made their appearance. In carbonic acid, carbonic oxyd, olefiant gas, marsh-gas, and cyanogen, the special lines due to the metal were produced, but in each a series of identical lines appeared, and these new lines were referable to the carbon contained in each of these gases. Each gas exhibits special lines which are continued across the spectrum, and are never interrupted like those of the metals.

The author observed that many of these gases, such as protoxyd of nitrogen, chlorhydric and sulphurous acid, presented a considerable obstacle to the passage of the sparks from the induction-coil.

4. *On the Long Spectrum of Electric Light;* by Professor George G. Stokes.—The author's researches on fluorescence had led him to perceive that glass was opaque for the more refrangible invisible rays of the solar spectrum, and that electric light contained rays of still higher refrangibility, which were quite intercepted by glass, but that quartz transmitted these rays freely. Accordingly he was led to procure prisms and a lens of quartz, which, when applied to the examination of the voltaic arc, or of the discharge of a Leyden jar, by forming a pure spectrum and receiving it on a highly fluorescent substance, revealed the existence of rays forming a spectrum no less than six or eight times as long as the visible spectrum. This long spectrum, as formed by the voltaic arc with copper electrodes, was exhibited at a lecture given at the Royal Institution in 1853; but the author, for reasons he mentioned, did not then further pursue the subject. * *

* * Among the metals examined, the author had found aluminum the richest in invisible rays of extreme refrangibility; and accordingly aluminum electrodes were employed when the deportment of such rays had to be specially examined. As the bright aluminum lines of high refrangibility do not appear to have been taken by photography, a

drawing of the aluminum spectrum is given, with zinc and cadmium for comparison.

The author has also described and figured the mode of absorption of the invisible rays by solutions of various alkaloids and glucosides. Bodies of these classes, he finds, are usually intensely opaque, acting on the invisible spectrum with an intensity comparable to that with which coloring matters act on the visible. This intensity of action causes the effect of minute impurities to disappear, and thereby increases the value of the characters observed. It very often happens that, at some part or other of the long spectrum, a band of absorption, or maximum of opacity, occurs; and the position of this band affords a highly distinctive character of the substance which produced it.

Among natural crystals, besides the previously known yellow uranite, the author found that in adularia, and feldspar generally, a strong fluorescence is produced under the action of the rays of high refrangibility, referable not to impurities, but to the essential constituents of the crystal. A particular variety of fluor-spar shows also an interesting feature, though in this case referable to an impurity, exhibiting a well-marked reddish fluorescence under the exclusive influence of rays of the very highest refrangibility. This property renders such a crystal a useful instrument of research.

With some metals broad, slightly convex electrodes were found to have a great advantage over wires, exhibiting the invisible lines far more strongly, while with some metals the difference was not great.

The blue negative light formed when the jar is removed, and the electrodes are close together, was found to be exceedingly rich in invisible rays, especially invisible rays of moderate refrangibility. These exhibited lines independent of the electrodes, and therefore referable to the air. This blue light has a very appreciable duration, and is formed by what the author calls an arc discharge.

The paper concludes with some speculations as to the cause of the superiority of broad electrodes, and of the heating of the negative electrode.

5. *On the Reflexion of Polarized Light on Polished Surfaces;* by the Rev. Samuel Haughton.—When a plane-polarized beam of light is incident on a polished surface at a certain angle of incidence, and polarized in a certain azimuth, the reflected beam of light is circularly polarized.

The tangent of this angle of incidence is called by the author the Coefficient of Refraction, and upon it appears to depend the *brilliancy* of a polished surface.

The cotangent of the azimuth of incident polarization is called the Coefficient of Reflexion, and upon it appears to depend the rich *lustre*, strikingly exhibited in polished copper and gold.

The paper contains an account of the experiments made to determine, with precision, these constants for the following substances:—

A. *Transparent Bodies.*

1. Munich glass (*a*).
2. Munich glass (*b*).
3. Paris glass.
4. Fluor-spar.
5. Glass of antimony.
6. Quartz crystal.

B. *Pure Metals.*

1. Silver.
2. Gold.
3. Mercury.
4. Platinum.
5. Palladium.
6. Copper.
7. Zinc.
8. Lead.
9. Bismuth.
10. Tin.
11. Iron and steel.
12. Aluminum.

C. *Alloys.*

1. Copper and tin (speculum metal).
2. Copper and zinc (10Cu+Zn).
3. " " (9Cu+Zn).
4. " " (8Cu+Zn).
5. " " (7Cu+Zn).
6. " " (6Cu+Zn).
7. " " (5Cu+Zn).
8. " " (4Cu+Zn).
9. Copper and zinc (3Cu+Zn).
10. " " (2Cu+Zn).
11. " " (Cu+Zn).
12. " " (Cu+2Zn).
13. " " (Cu+3Zn).
14. " " (Cu+4Zn).
15. " " (Cu+5Zn).

The determination of the optical constants of these substances leads to many interesting conclusions; among which the following may be stated:—

1. That Transparent bodies, as well as metals, possess a coefficient of reflexion, which is sometimes very sensible, although there are bodies in which it is very small.

2. That *Silver* is the only substance which possesses the qualities of *brilliancy* and *lustre*, represented by the coefficients of refraction and reflexion, in a high degree.

3. Of the metals which have high *brilliancy* and little *lustre* may be named *Mercury*, *Palladium*, *Zinc*, and *Iron*.

4. Of the metals which have high *lustre* and little *brilliancy* there are only two, *Gold* and *Copper*.

5. Results of the highest interest appear from an examination of the optical constants of the alloys of copper and zinc, which cannot be given in an abstract.

6. In the details of the several experiments, the author calls attention to several remarkable laws, or indications of laws, which appear to him to require some notice from theorists.

a. When the azimuth of the incident beam is less than the circular limit, the axis major of the reflected ellipse, at the principal incidence, lies in the plane of incidence; but when the azimuth is greater than the circular limit, it is perpendicular to the plane of incidence, and as the incidence varies, the axis major twice approaches to a minimum distance from that plane.

b. There appears to the author to be some indication in the experiments on metals, that the quantity known to theorists as $\left(\frac{J}{I}\right)$ is not a function of the incidence only; a conclusion which, if correct, would require the intervention of a third wave suppressed, or some such theoretical supposition, to account for it.

SCIENTIFIC INTELLIGENCE.

I. PHYSICS.

1. *Gemsbart Electroscope.*—Prof. KOBELL made an interesting communication to the mathematico-physical class of the Bavarian Academy of Sciences, in their session of Jan. 10th, 1863, on the electroscopic properties of the so-called "Gemsbart," a name given by the Alpine hunters to the long hairs which grow along the back of the male Chamois in the autumn of the year, and are well known as the trophies with which the Tyrolese hunter decorates his hat. These hairs from a four years' buck reach the length of six inches and more, are very fine and generally terminate in a white point. If several are taken together by the root and stripped through the fingers, they repel each other to a great distance; if held by the points and rubbed towards the roots, a similar but weaker effect is produced. The electricity in the former case is positive, in the latter negative, and the hairs thus become respectively + and — indicators. This property adapts them in an eminent degree to the purposes of an electroscope, being more sensitive than the usual Hauy's apparatus. For use they are fastened with wax to a glass or sealing-wax rod.

If the plane of a crystal, in which electricity has been excited by friction, pressure or heating, repels one of these indicators, the kind of electricity is *eo ipso* determined, and also the fact that the body is an insulator. If *one* of the indicators is attracted, the electricity is either the opposite, (in which case the other indicator is to be applied) or the body is non-electric. If both are attracted, the body is either non-electric, or a good conductor, in which latter case it must be insulated. To determine the poles of a thermo-electric crystal, it suffices to hold it between the thin points of an elastic steel forceps ending in a wooden handle, and then to examine it with the stronger + indicator, which has to be drawn through the fingers from time to time. By this method the poles of small crystals of boracite, thin needles of scolezite, calamine and Brazilian topaz were easily determined, small crystals generally giving a more constant and decisive reaction than larger ones.

Highly electric crystals, as those of tourmaline, often show the poles plainly, even after they have become perfectly cool externally. For their examination it is most convenient to attach the hair in the middle with wax to a Hauy's needle, and at right angles to the same, and then in the aforesaid manner to excite the opposite electricities in the two ends, when the brass needle is immediately set in motion upon the approach of the electric tourmaline, and the poles can be distinctly shown by alternate repulsion and attraction.

The two-fold electricity of such a hair is evidently connected with its structure, for it is smooth from root to point, and feels rough in the opposite direction. This is corroborated by the fact, that, if a hair by frequent use has been made smooth in the latter direction, it changes its negative electric character into the positive. This occurs after about one hundred experiments, when it can no longer be used as an — indicator. Prof. Bischof, who examined the hairs with the microscope, states that they have a highly developed epithelium, while the fibrous cortical substance is subordinate, and is almost entirely wanting towards the lower part, where it is replaced by the epithelium. The cortical fibres, as usual,

contain the pigment. They are distinguished by the large proportion of pith, which begins a short distance from the point and very soon constitutes almost the entire thickness of the hair. The pith consists of large polygonal cells. Thus the opposite electrical state of the two ends may possibly be caused by the prevalence of the cortical fibre in the upper and of the pithy substance in the lower part, this pith being filled with air cells.

Prof. Kobell considers a closer study of the electrical properties of crystals, induced by friction, very desirable; he recommends the use of deer-skin stretched over a wooden pestle as a rubber, giving it the preference over woolen and silk on account of the frequent admixture of the latter with other fibre. Employing this rubber, (and in case of thin laminæ, as those of mica, merely the dry fingers,) and then examining the electricity with the "Gemsbart," he has made the following electrical groups of minerals:

I Group. *Good insulators*

When rubbed attract the indicator.

1. Div. *Electro-positive insulators*
repel the + indicator.

Calcite, aragonite, fluor, barytes, glauberite, gypsum, anhydrite, apatite, quartz, topaz, emerald, grossular, idocrase, kyanite, orthoclase, albite, tourmaline, axinite, zircon, muscovite (Grafton, N. H.), spinel, alum, rock-salt, etc.

2. Div. *Electro-negative insulators*
repel the − indicator.

Talc, sulphur, orpiment, amber, asphaltum.

II Group. *Good conductors.*

Do not attract the indicator, and are coated by metallic copper, when immersed with a zinc holder in a solution of sulph. copper. Graphite, gold, silver, galena, pyrites, mispickel, chalcopyrite, cobaltine, smaltine, magnetite, etc.

III Group. *Bad conductors and bad insulator as compared with Group II.*

Do not attract the indicator, or only feebly, and are not coated with copper if treated as in Group II.

Diamond, celestine, almandine, melanite, biotite and phlogopite, ripidolite and clinochlore, pennine, analcime, sphene, stibnite, hematite, franklinite, zinkenite, jamesonite, chromic iron, red copper, pyrolusite, manganite, psilomelane, hausmannite.

To determine the kind of electricity of Groups II and III, the minerals must be insulated; this is readily done by fastening them with wax on the end of a glass rod of sufficient diameter, taking care that the face to be rubbed projects far enough beyond the wax, to prevent the rubber from coming in contact with the latter.

In the examination of small crystals, it is often convenient to mount them on wax, thereby insulating them to determine whether the mineral belongs to Group I, or is near it, it is only necessary, after rubbing it, to touch a part of the crystal with the finger, when if it be a good insulator it will not loose its electricity. W. T. R.

2. *Conductibility and specific heat of Thallium.*—De la Rive has compared the conducting power of thallium with that of mercury by the method of Wheatstone. The density of the metal was found to be 11,853 at 11° C., which agrees well with the determination of Lamy, namely, 11,862 at 0°; the density of the wire is 11,808. The conducting power of silver being taken as 100, that of mercury is 1·63, and that of thallium 8·64, a value which lies between those for lead and tin, and which is much lower than the corresponding values for the alkaline metals. The specific heat of thallium was found by Regnault to be 0·03355, as a mean of two experiments. The product of this number by the equivalent 204, gives 85·55, half of which is 42·77, so that thallium in its thermic relations is associated with the alkaline metals, and the formula of its protoxyd should be T_2O if potash is written K_2O.—*Comptes Rendus*, lv, 887 and lvi, 588. W. G.

II. CHEMISTRY.

1. *On the tungstates, fluo-tungstates and silico-tungstates.*—Marignac has communicated the results of an extended study of the tungstates, a class of salts exhibiting remarkable anomalies in constitution. The author finds the results of Scheibler in regard to the meta-tungstates to be correct. These salts may all be expressed by the general formula $RO, 4WO_3 + x\,aq$, and the acid in them is not precipitated by stronger acids. The ordinary tungstates contain the insoluble modification of tungstic acid. They are frequently very complex in constitution, and appear to be in these cases compounds of bi-tungstates and ter-tungstates. One class of these salts was distinguished by Laurent under the name of paratungstates, and possess, according to that chemist, the general formula $5RO, 12WO_3$. Lotz and Scheibler gave them the general formula $3RO, 7WO_3$, but Marignac is disposed to return to the formula of Laurent, considering the compounds, however, as double salts. The author did not succeed in obtaining fluo-tungstates free from oxygen compounds, agreeing in this respect with Berzelius, whose results were expressed by the general formula $RO, WO_3 + RF, WF_3$. According to Marignac, the same two salts combine in other proportions also, but the most remarkable circumstance is the isomorphism of the copper-salt, $CuO, WO_3 + CuF, WF_3$, with the fluo-silicate, fluo stannate and fluo-titanate of copper, the last having the formula CuF, TiF_2. This isomorphism becomes intelligible when the formulas are written $Cu_2W_2O^2F_4$, and $Cu_2Ti_2F_6$, so that it must be admitted that fluorine and oxygen may in certain cases replace each other *atom* for *atom*, though not equivalent for equivalent, and further that Berzelius' mode of viewing the constitution of the salt cannot be correct, since it furnishes no explanation of the isomorphism. The silico-tungstates form a new class of salts, and may in general be easily obtained by boiling a solution of an acid tungstate with gelatinous silicic acid: they are easily soluble, and usually crystallize well. They are not decomposed by boiling with chlorhydric acid or by evaporation to dryness with it, but are simply converted into acid silico-tungstates. Marignac considers the salts of silico-tungstic acid to contain 10

eqs. of tungstic to 1 eq. of silicic acid: the neutral salts contain four equivalents of base, but it is perhaps more proper to double the formulas, and give the neutral salts the general formula $8RO, 20WO_3, 2SiO_2$, in order to include the acid and double salts.—*Comptes Rendus*, lv, 888, *Ann. der Chemie und Pharm.*, cxxv, 362. W. G.

2. *On the preparation and properties of metallic rubidium.*—Bunsen has prepared metallic rubidium, by igniting in a proper apparatus the carbonized bitartrate of the oxyd. From 75 grammes of the salt, 5 grammes of metal were obtained in a single mass. Rubidium is very brilliant, like silver, white with a scarcely perceptible tinge of yellow. In the air it oxydizes instantly to bluish-grey suboxyd, and takes fire, after a few minutes, much more easily than potassium. At $-10°$ C. it is still as soft as iron: it melts at 58°·5 C., and below a red heat is converted into a blue vapor with a shade of green. According to Bunsen, the true fusing point of sodium is 95°·6 C., and that of potassium 62°·5 C.; the latter does *not* pass through an intermediate pasty condition in fusing. The density of rubidium is about 1·52. It is considerably more electro-positive than potassium, takes fire upon water and burns with a flame which cannot be distinguished from that of potassium by the eye. Rubidium burns with brilliancy in chlorine and in the vapor of bromine, iodine, sulphur, and arsenic.—*Ann. der Chemie und Pharmacie*, cxxv, 367. W. G.

3. *On the preparation and properties of metallic magnesium.*—H. Sainte-Claire Deville and H. Caron have given a description of the most recent and improved method of preparing magnesium, and of the properties of the metal in a state of absolute purity. Their results are by no means wholly new, but are interesting as being complete in detail. To obtain pure magnesium, the authors in the first place weigh and mix rapidly 600 grammes of pure chlorid of magnesium obtained by Liebig's method, 480 grammes of pure fluorid of calcium, and 230 grammes of metallic sodium carefully cleaned and cut into small pieces. The mixture is projected into a red hot earthen crucible, which is then covered; a weight is placed on the cover to hold it in its place. When the reaction is finished, the mass is stirred with a clean iron rod, and removed from the fire. Pure and dry fluorid of calcium in fine powder is then thrown in, and the stirring repeated. The iron rod gradually collects all the globules of magnesium into a single mass, which collects near the surface of the heavier saline mass. On breaking up this mass, after cooling, about 92 grammes of magnesium may be collected, which is nearly $\frac{3}{4}$ of the theoretical quantity. The scoria from which the magnesium has been separated may be fused a second time and more magnesium obtained, so that sometimes 100 grammes of sodium will give 45 grammes of raw magnesium. The globules of magnesium may be brought into a single mass by fusing together 60 grammes of chlorid of sodium and 75 grammes of chlorid of potassium (Wöhler's flux), breaking up the cooled mass, and fusing with the magnesium. The metal floats at first, but, as the flux cools, the magnesium becomes specifically heavier, sinks to the bottom of the crucible, and forms a single mass. As thus prepared, magnesium contains carbon, silicon and nitruret of magnesium. To obtain the pure metal, the authors distil the raw product in tubes of gas-retort carbon, surrounded by earthenware tubes, and traversed by a current of hydrogen.

The pure metal has a density of 1·75, is very ductile, and gives plates of great lustre and of a slightly bluish or violet tint when burnished. The surface tarnishes in the air, but not more rapidly than zinc, and the oxydation is never very deep. Magnesium fuses at about the fusing point of zinc: at a little higher temperature it burns with an intensely brilliant flame, as already observed by Bunsen. The light of burning magnesium contains all the rays peculiar to the metal, without the inversion observed by Fizeau in the combustion of sodium.—*Annales de Chimie et de Physique*, lxvii, p. 340. W. G.

4. *On the chemical constitution of the American rock oil.*—Schorlemmer has examined the oils obtained by the distillation of cannel coal at low temperatures, the mixture of which is known in the United States as kerosene. The oil in question is found to contain a series of homologous hydrocarbons of the general formula CnHn+2 and consisting of the hydrurets of the alcohol radicals. The oil, which boils below 120° C., contains the four hydrurets,

$C_{10}H_{12}$,	hydruret of amyl,	boiling at	39° C.,
$C_{12}H_{14}$,	" hexyl,	"	68° C.,
$C_{14}H_{16}$,	" heptyl,	"	98° C.,
$C_{16}H_{18}$,	" octyl,	"	119° C.

The author found precisely the same products in the American petroleum or rock oil. The oils are first purified by strong nitric acid, which leaves the greater part unattacked, but removes benzole and toluole. After washing, drying over caustic potash, and distillation with sodium, the four hydrurets already mentioned were obtained as in coal tar. Benzole and toluole are found in larger proportion in cannel coal tar than in petroleum. —*Proc. Manchester Phil. Soc.*, March 11, 1863. W. G.

5. *On some new organic compounds of silicon.*—Friedel and Crafts have prepared, in the laboratory of Wurtz, some interesting compounds of silicon with organic radicals. By heating together silicic ether, $\left.\begin{matrix}\overset{iv}{Si_2}\\(C_4H_5)_4\end{matrix}\right\}O_8$, and bichlorid of silicon, Si_2Cl_4, the authors obtained a new compound having the formula $\left.\begin{matrix}\overset{iv}{Si_2}\\3C_4H_5\\Cl\end{matrix}\right\}O_6$. This body boils at about 156° C.; the density of its vapor is 7·05 by experiment and 6·87 by theory. It may be regarded as silicic ether in which one equivalent of the body, $C_4H_5O_2$, is replaced by one equivalent of chlorine. A second product is found at the same time, the formula of which is $\left.\begin{matrix}\overset{iv}{Si_2}\\2(C_4H_5)\\Cl_2\end{matrix}\right\}O_4$ and which is therefore the dichlorhydrine of silicic ether. Equal equivalents of the mono-chlorhydrine and the amylic alcohol give a new ether, the formula of which is $\left.\begin{matrix}\overset{iv}{Si_2}\\3(C_4H_5)\\C_{10}H_{11}\end{matrix}\right\}O_8$, that is, normal silicate of ethyl,

in which one atom of ethyl is replaced by one atom of amyl. When chlorid of silicon and zinc-ethyl are heated together in a sealed tube, a limpid liquid is obtained, which is insoluble in water, and not acted upon by a concentrated solution of potash or by nitric acid. This liquid is silicon-ethyl, the formula of which is $\left.\begin{matrix}\overset{iv}{Si} \\ 4(C_4H_5)\end{matrix}\right\}$. The density of the vapor of silicon ethyl is 5·13 by observation and 4 99 by theory: it corresponds in constitution to distannethyl, $Sn_2(C_4H_5)_4$, and diplumbethyl, $Pb_2(C_4H_5)_4$. The authors promise a more extended study of this body, which, as the first organic compound consisting only of silicon, carbon, and hydrogen, is of much interest. The formula SiO_2 or Si_2O_4 for silicic acid appears to be at last definitively established.—*Comptes Rendus*, lvi, 590. W. G.

6. *On the coloration of flame by phosphorus and its compounds.*—CHRISTOPLE and BEILSTEIN find that, when phosphorus is added to the materials for preparing hydrogen, the flame takes a beautiful emerald-green color. With the spectroscope, this flame gives two magnificent green lines having about the same degree of intensity, and a third rather less distinct between the two first and the sodium line D. Red phosphorus, phosphorous and hypophosphorous acids give the same result. This reaction is extremely sensitive, and may be used in cases of poisoning by phosphorus, and in detecting the presence of minute quantities of phosphorus in iron.—*Comptes Rendus*, lv, 399. W. G.

ANALYTICAL CHEMISTRY.

7. *On estimation of nitric acid by conversion into ammonia.*—On the 14th of June, 1848, J. C. Nesbit read before the Chemical Society of London (*vide* its *Quarterly Journal*, vol. i, pp. 281–5) a paper "On a New Method for the Quantitative Determination of Nitric Acid, and other Compounds of Nitrogen." Nesbit founded his method on the old observation that "nascent hydrogen" or the metals tin, iron, and zinc, when undergoing solution in a dilute acid, decompose nitric acid with formation of ammonia. He especially refers to the statement of Kuhlmann that the addition of a nitrate, to a mixture of zinc or iron with dilute chlorhydric or nitric acid, causes the evolution of gas to cease for a while, a salt of ammonia being at the same time produced according to the equation—

$$NO_6H + 8H = NH_3 + 6HO.$$

Nesbit ascertained that, by the observance of certain precautions, the whole of the nitrogen of nitric acid or of nitrates may be thus transferred to hydrogen, and obtained as ammonia. His directions are as follows; "If ten grains of salt, such as nitrate of potash, be taken for analysis, about 200 or 300 grains of thin clear (clean?) fragments of zinc are put into a small flask, to which a portion of water is added. From half to three quarters of an ounce of chlorhydric acid, sp. gr. 1·17, must be poured out into a small measure, and about one-tenth part added to the zinc and water. When effervescence has fairly commenced, a portion of the nitrate, previously dissolved in a small quantity of water, is added to the mixture. The temperature of the whole must, if necessary, be kept low, by placing the vessel in cold water. After a short period, a little more

acid is added, and then a little nitrate, until all the solution of the nitrate with the washings is poured in, and about one-fourth of the acid is left. Care should be taken that, for the first hour, the effervescence is slow. When the whole of the solution of the nitrate is poured in, the remainder of the acid must be added from time to time, and the whole left until effervescence ceases. The liquid is then carefully separated from the undissolved zinc, which is well washed with the smallest quantity of water, and the whole distilled with hydrate of lime, the ammonia being collected in a proper condenser. The great danger to be avoided consists in allowing the hydrogen to be liberated too rapidly, by means of which so much heat is generated as to cause a portion of the nitrogen to escape as binoxyd of nitrogen." Nesbit estimated the ammonia by a volumetric method. He gives the results of sixteen determinations of nitric acid, by five different operators, in nitre both pure and mixed with 6 to 9 times its weight of common salt, and in the nitrates of baryta and lead. In each case, the accuracy of the estimation left nothing to be desired.

Nesbit's method has been employed in this laboratory with satisfaction, though several trials were requisite for learning the precise method of procedure.

We have thus noticed the method of Nesbit, because it has not, to our knowledge, been described in any treatise on chemical analysis, and because, since its publication, others have proposed methods based on the same principle which are more or less worthy of notice.

Several years after the method of Nesbit was published, Martin gave out the same process as original, *Comptes Rendus*, xxxvii, 947, with this important difference, that the necessary precautions are indistinctly stated. He directs that pure zinc, in quantity four to five times as much as the nitric acid to be reduced, be placed in a glass, together with the salt or the solution to be analyzed, and pure sulphuric or chlorhydric acid be repeatedly added. Martin mentions that uric acid, sulphate of quinine, gelatine, and "the nitrogenous substance which occurs in water," do not affect the accuracy of the process, though in presence of gelatine the reduction proceeds very slowly.

In 1861, Schulze, of Rostock, proposed to convert nitric acid into ammonia by the action of sodium-amalgam or of platinized zinc in presence of excess of alkali (*Chem. Centralblatt*, 1861, pp. 657 and 833). The manipulations described by Schulze for obtaining good results are somewhat troublesome, and the method, according to Wolff, often practically fails from the slowness of the reduction. Wolff recommends to modify the process by employing a mixture of finely rasped zinc and iron-filings with a soda lye made by dissolving one part of hydrate of soda in eight parts of water, (*Chem. Centralblatt*, 1862, p. 379,) Runge having long ago observed that zinc in contact with iron, in alkaline liquors, suffers solution to a far greater degree than when in contact with platinum. To determine the ammonia, Wolff employs a method elaborated by Knop and himself, in which the nitrogen of the ammonia is liberated by a mixture of hypochlorite of soda and bromine, and estimated by measurement.

The analysis of nitrates by reduction with a mixture of zinc and iron in alkaline liquid has also been the subject of nearly cotemporary study

with A. Vernon Harcourt and Mr. Siewert, *Ann. Ch. u. Ph.*, 1863, p. 293. Harcourt obtained excellent results, but, owing to the difficulty of distilling ammonia from a solution containing oxyd of zinc and caustic alkali without some of the latter being carried over, was obliged to employ a complicated apparatus. Siewert presents the simplest method of operating. For the reduction of 0·54 grm. of NO_5, he uses 4 grm. of iron and 8–10 grm. of zinc filings, 16 grm. of solid caustic potash, and 100 c. c. of alcohol of sp. gr. 0·825. The substitution of alcohol for water prevents the troublesome frothing of the mixture when undergoing distillation. The apparatus employed is an evolution flask of 300 to 350 c. c. capacity, which is connected by a rather long doubly-bent tube with a condensing arrangement consisting of two flasks of 200 c. c. content each, that are also connected together by a doubly-bent tube. The tube uniting the evolution flask with the first condensing flask has its extremities cut obliqnely so that no adhering drops of liquid can be carried mechanically either way, and they just pass through the corks which fit them vapor-tight into the flasks. The tube that joins the two condensing flasks, on the other hand, passes nearly to the bottom of these flasks. In the middle flask a quantity of standard acid, more than enough to fix the ammonia yielded by an experiment, is placed, and the mixture of nitrate, potash, zinc, iron, and alcohol being brought into the evolution flask, the corks are tightly fixed, and the gas evolution is assisted by a very gentle heat. In half an hour, the ammonia begins to pass over with alcohol vapor. When all the alcohol has gone over, only traces of ammonia usually remain in the evolution flask. These may be removed, either by continuing the heat until water distils, great care being taken that frothing or spirting do not endanger the result, or better, 10 to 15 c. c. of alcohol are poured into the evolution flask, quickly raising the cork for that purpose. If needful, the addition of alcohol may be repeated. After distilling 2 to 3 hours, the ammonia in the receivers may be estimated by the approved volumetric processes.[1]

Siewert gives the results of a number of determinations which indicate that his method is satisfactory, and its simplicity gives it preference over all the others heretofore devised which are based on the same principle.

It is scarcely needful to remind the analyst that several of the reagents employed are liable to contain either ammonia or nitric acid, and that their purity must be carefully attended to. S. W. J.

III. MINERALOGY AND GEOLOGY.

1. *Annual Report of the State Geologist of California for the year* 1862, 12 pp., 8vo. San Francisco, 1863.—From this brief letter, addressed by Prof. Whitney to the Governor of California, and from a further communication made to the California Academy of Natural Sciences, we abstract the following facts in regard to the progress of the survey: "In the topographical department, a series of maps, forty-nine in number, has been compiled by Mr. Hoffmann from the original documents at the United States Surveyor General's Office: the scale of these is half an inch to the mile. They contain a compilation of nearly all that is known

[1] See this Journal, [2], xxxv, 279.

at that office in regard to the geography of the State. The maps, as thus blocked out, have been used by us in the field, by filling in the topography wherever our route has laid.

The maps which have been or are now being prepared for publication are:

1st. A map of the vicinity of the Bay of San Francisco, on a scale of half an inch to the mile, four feet by three; it extends from near Santa Cruz on the south to Napa on the north, and from the Pacific to Corral Hollow, east and west. The area of land which it covers is 4,248 square miles, which is just twice that of the State of Delaware, and only lacks two hundred square miles of equaling that of Connecticut. As near as can be ascertained, it contains one-third of the population of the State, and has about thirty inhabitants to the square mile—the average density of the population of California being but little over two to the square mile. This map, on which all the details of the topography are given, as minutely as the scale allows, is nearly completed, and will be soon ready for the engraver.

2d. A detailed map, on a scale of two inches to the mile, of the vicinity of Mount Diablo; this is about two and one-half by three feet in dimensions, and includes the most important coal mining district yet known to exist in the State. The map can be made ready for the engraver in a few days.

3d. A map of the Coast Ranges, from the Bay of Monterey south to Santa Barbara. It is about three feet by two and one-half in dimensions, is on a scale of six miles to the inch, and embraces about 16,000 square miles of territory. To complete it will require about another year's work in the field with two sub-parties.

4th. Map of the Washoe silver-mining region—three and one-half by two and one-half feet in dimensions, on a scale of two inches to the mile—and extending over all the important mining ground of the district. This map is from an accurate trigonometrical survey by V. Wackenreuder; it is nearly completed.

5th. Map of the Comstock Lode, on a scale of four hundred feet to the inch, completed.

6th. Map of the central portion of the Sierra Nevada; scale not yet determined on. Extensive surveys have been made by Mr. Wackenreuder for this part of the work, and these will be continued during the present season.

Of the above mentioned maps, Nos. 1 and 2 will accompany the first volume of the Report. Nos. 4, 5, and probably 6, the second volume.

It is intended, if the survey is carried to completion, to construct a final map of the State on a scale of six miles to the inch, in nine sheets, each about three feet square.

In addition to the regular topographical work, an extensive series of barometrical observations has been made, for the determination of altitudes, and some two hundred and fifty important points have been ascended and measured. The most interesting operation in this department was the determination of the height of Mount Shasta, which, by an elaborate series of observations, we found to be 14,440 feet above the sea level. This is the first of the lofty volcanic peaks of the Sierra Nevada which has been accurately measured.

In the department of geology proper, our explorations have extended over portions of forty of the forty-six counties into which the State is divided; and when it is remembered that the average size of a county is equal to half that of the State of Massachusets, (California having just twenty-four times the area of that State,) some idea of the magnitude of our work may be obtained. The chain of the Sierra Nevada may be parallelized with that of the Alps for extent and average elevation; while the Coast Ranges are nearly as extensive as the Appalachian chain of mountains.

We have obtained a pretty clear idea of the general structure of the Coast Ranges from Los Angeles to Clear Lake; the vicinity of the Bay of San Francisco has been worked out in considerable detail, including all of San Francisco, San Mateo, Santa Clara, Alameda, Contra Costa, and Marin Counties, with portions of Santa Cruz, Solano, Napa, and Sonoma. Considerable field-work has been done in the Sierra Nevada, chiefly in the lower portion of the range between Mariposa and Shasta Counties. Our observations have also been extended to the Washoe Region, and we have received considerable collections of fossils from the Humboldt Mining District, (known by this name on the Pacific Coast, but designated on Warren's Map as the "West Humboldt River Range," and in longitude 118°,) by which we have been able to fix the age of the formations in that region.

Mr. Gabb has been chiefly occupied, the past year, in figuring and describing the Cretaceous fossils of the Coast Ranges and the foot-hills of the Sierra, of which he has nearly two hundred new species ready for publication. He has also described the Triassic fossils, collected by the Survey at Washoe, and by Gorham Blake, Esq., in the Humboldt Range. The fossils older than the Trias have been referred to Mr. Meek for investigation. A portion of the fossil plants have been placed in the hands of Dr. J. S. Newberry for description.

It is to the department of General Geology that, up to the present time, by far the greater portion of our attention has been given, since the first thing required in a geological survey is a knowledge of the general geological structure of the State, the age of the various formations which occur in it, and their range and extent, or the position which they occupy on the surface, and their relations to each other. Each group of strata, thus determined by its lithological peculiarities, and by the fossils which it contains, is then to be laid down upon the map, in the position in which its outcrop occupies on the surface. The general character of the minerals and ores which occur in each formation or group of strata having been thus determined, the details of their mode of occurrence, their relative abundance, and the facilities which may exist in each separate district for making them economically available must, after the preliminary general work has been done, be the object of more special and detailed examinations. It is not, however, the business of a geological surveying corps to act, to any considerable extent, as a prospecting party; to do this, would require that we should confine our operations to a very limited area; the labors of the whole corps for an entire season would not suffice to thoroughly prospect more than a few hundred square miles in a very rich mineral region, and we should have

often to engage in expensive mining operations to decide what was really of permanent value. It is our task, rather, to limit the field of research, and to show to others where their labors will be best bestowed, preventing foolish expenditures of time and money in searching for what our general geological investigations have determined not to exist in sufficient quantity, in certain formations, to be worth working. Especially in the first years of our work, in a State of such an immense area as California, our labors have more the character of a geological reconnoissance than of a detailed survey.

Already, however, during the progress of our work, a large amount of information has been collected in regard to the mode of occurrence and abundance of the useful ores and minerals of this State and the adjoining Territories. The principal deposits of coal have been carefully examined, and their geological position ascertained. Most of the important quartz mines of the State have been visited by Mr. Ashburner, and a large amount of information has been collected by him, preparatory to an elaborate investigation and report on this important branch of the industry of the Pacific Coast. Considerable work has been done, preliminary to a full report on the geology, mineralogy, and metallurgy of the Washoe region.

In the department of botany and agricultural geology, the work has thus far been chiefly confined to collecting the plants of the State.

Extensive duplicate suites have been preserved, both for study and for exchange, the specimens now collected amounting to not less than twelve thousand or fifteen thousand in number, and embracing probably half of all the species described from the State, besides many new and undescribed ones. The collections have been made by Professor Brewer while engaged in geological explorations, at a very trifling expenditure of time and money.

In the department of agriculture proper, less has been done, owing to limited means. Partial preparation was made for investigating the subject of grape culture, and the production of wines; but discontinued from the same cause. Especial attention has been paid to our native forage plants, to aid in devising some means of arresting the rapid decrease of forage in this State, and correspondence entered into to obtain all possible information on this subject from other regions whose climates are similar to our own.

In the zoological department—in charge of Dr. J. G. Cooper, who has been employed about half the time since the Survey was commenced—the annexed table gives a succinct idea of what has been accomplished, up to the close of the year 1862, in the way of collecting.

Class.	Number of species in the collection.	Of which there are new to California.	Believed to be new, or undescribed.	Other Californian species not yet collected.	Total No. credited to California.	Of which there are found east of the Mississippi.
Mammals	32	10	3	45	77	14
Birds	170	28	4(?)	150	320	141
Reptiles	36	6	3	9	45	0
Fishes	58	16	16	75	133	0
Mollusks	335	123	128	65	400	0(?)

Of Articulates and Radiates no statistics can be given for want of works especially devoted to the California species.

From this it appears that, notwithstanding the large collections made by government expeditions and by individuals, during the last ten years, which have been elaborately described in the Pacific Railroad and Mexican Boundary Reports, the Smithsonian publications, and various other works, we have been able to add materially to the known Fauna of California, and of the country at large, even among the highest and best known classes.

Arrangements have been made for having the collections in natural history referred to the highest authorities in each branch, and portions of our materials have already been placed at the disposition of eminent men in Europe and the United States for examination and description.

Deferring the fitting up of a laboratory, and the engaging of a special assistant in the chemical department, until a suitable permanent place could be provided in the State Museum building, Mr. Ashburner went East in the spring of 1862, and commenced the examination of some of the ores and minerals of the State in the laboratory of the Sheffield Scientific School of Yale College, under the direction of Professor Brush, who has charge of the metallurgical department of that institution. The reduction of the appropriation to fifteen thousand (15,000) dollars for the year, made it necessary to suspend this work soon after it was commenced, in order that the whole force of the Survey might be concentrated on the field operations.

A small sum has been allowed to Mr. F. H. Storer, of Boston, for a chemical investigation of the bituminous substances found in different parts of the State. His researches will probably be embodied in the first or second volume of the annual reports. Qualitative examinations, as well as a few quantitative ones, have been made at the office of the Survey, of specimens which have been collected. A considerable number of coals have been analyzed. Information in regard to ores and minerals has been given to a large number of persons who have applied for the same by letter or otherwise, as will always be done when practicable."

The full results of this work, together with what may be finished during the present year, will probably be published in 1864. These results will make two, or perhaps three, royal octavo volumes, and will be illustrated with maps, sections, plates of fossils, etc. The appropriation for the present year is $20,000, an amount entirely inadequate to carry on the Survey in as complete a manner as contemplated in the Act of the Legislature. We are confident that the people will soon see the great practical advantage to be derived from this Survey in developing the immense mineral and agricultural resources of California, and will make liberal appropriations to have it conducted in a manner alike beneficial and honorable to the State. We have no doubt that, in the establishment of such a laboratory as proposed by Prof. Whitney for the investigation of questions in regard to the composition and metallurgical treatment of ores, more than enough would be saved in one year to pay the whole expenses of the Geological Survey. G. J. B.

2. *Discovery of Childrenite at Hebron in Maine.*—The "minute prismatic crystals of a hair-brown mineral" mentioned in my article on

amblygonite[1] prove, on further examination, to be childrenite. Mr. O. D. Allen has recently visited the locality, and obtained a sufficient quantity of the mineral to determine its specific characters, and a description of these, including crystallographic measurements, by Prof. J. P. Cooke, Jr., will appear in the next number of this Journal. G. J. Brush.

3. *On the Height of Mt. Shasta, California;* by J. D. Whitney, in charge of the Geological Survey of California.—A careful and elaborate series of barometrical observations by the State Geological Corps of California, made in September, 1862, has fixed the elevation of Mt. Shasta at 14,440 feet.[2] Previous to this, the height of Shasta had been variously estimated at from 13,905 to 18,000 feet. The number 13,905 was the result of a barometrical observation made by Mr. W. S. Moses, August 20th, 1861; 18,000 feet was the height as estimated by the Pacific Rail Road expedition, under Lieut. Williamson; Fremont's estimate was 15,000 feet, which is much nearer the truth than Williamson's. It is a very curious fact, that the height of Mt. Shasta, as given by the editor of Colton's Atlas and author of the article on California in the New American Cyclopædia is 14,390 feet, which is a very close approximation. Where these figures were obtained, I have been unable to ascertain.[3] It is pretty certain that they were not the result of any actual measurement, as it is known that Mr. Moses was the first person to ascend the mountain with a barometer.

4. *The Human Jaw at Abbeville.*—Vague and inaccurate statements have been going the rounds of some of the daily and weekly papers regarding the proceedings of the conference of men of science—English and French—which was engaged at Paris last week in investigating the case of the asserted discovery of a human jaw at Abbeville in the fossil state.

The following is a *résumé* of the proceedings:—The English deputies consisted of Mr. Prestwich, Dr. Falconer, Dr. Carpenter, and Mr. Busk, three of whom reached Paris on the 9th and the other on the 10th. The French members were, M. Milne-Edwards (President), M. de Quatrefages, M. Lartet, M. Delesse and M. Desnoyers. Three days were occupied in discussing the question of the flint *haches*, and in the examination of the jaw, the latter of which was taken up on the third day. No decisive result was arrived at. The English members of the Commission maintained the unauthentic character of all the flint *haches* which were yielded by the "black band," and nothing was established on the other side to shake their convictions. The jaw was sawn up and washed; the black coating was removed from it with the utmost facility; there was no infiltration of metallic matter through the walls of the bone, and the section was comparatively fresh looking. The tooth also was in every respect remarkably fresh-looking. The confidence of some of the French members of the Commission was seriously shaken by the characters yielded by the jaw, which, so far as internal evidence went, was want-

[1] This Journal, [2], xxxiv, 243.

[2] A sketch of this mountain, as seen from the South-west-by-South (compass course) is contained in this Journal, [2], vii, 251.

[3] Wilkes says "it is said to be 14,350 feet; but Lieut. Emmons thinks it is not so high."

ing in every appearance which commonly distinguishes fossil bones, and especially those found elsewhere in the Somme deposits. Had the conference been closed at Paris, it is not improbable that the result might have been the Scotch verdict of 'Not proven,' but, at the suggestion of the President, the Commission adjourned to Abbeville on the 12th, when the complexion of the case was at once altered.

Haches of the supposed unauthentic character were disengaged from the cliff of the gravel-pit of Moulin-Quignon, under the very eyes of the Commission, and direct testimony to the actual occurence of the jaw in the "black band" was brought forward to the conviction of the Commission. But there was not the same unanimity respecting the age of the jaw itself. Two of the English members of the Commission, Dr. Falconer and Mr. Busk, handed in notes of the opinions at which they had arrived on the general case. These we insert.—

"Abbeville, May 13, 1863.

"I am of opinion that the finding of the human jaw at Moulin-Quignon is authentic; but that the characters which it presents, taken in connexion with the conditions under which it lay, are not consistent with the said jaw being of any very great antiquity. H. Falconer."

"Abbeville, May 13, 1863.

"Mr. Busk desires to add, that although he is of opinion, judging from the *external* condition of the jaw, and from other considerations of a more circumstantial nature, that there is no longer reason to doubt that the jaw was found in the situation and under the conditions reported by M. Boucher de Perthes, nevertheless it appears to him that the *internal* condition of the bone is wholly irreconcilable with an antiquity equal to that assigned to the deposits in which it was found."

Mr. Busk of course refers here to the received opinion that the Moulin-Quignon deposits belong to the "high level" gravels of Mr. Prestwich, which are considered to be the oldest of the Somme beds.

From all this, it will be seen that the question of the relative antiquity of the relic is left open to discussion. It is manifest that the evidence was very conflicting; that it is in some respects of an incompatible character; and that a great deal still remains to be cleared up before the scientific world can arrive at a definite judgment on the case. We may further add, that the subject was again brought before the Academy of Sciences, on Monday last, in two distinct notes, by M. Milne-Edwards and M. de Quatrefages, who, we understand, did ample justice to the candor and frankness of their English opponents, and recognized in terms of praise the readiness which they had exhibited in proceeding to Paris when invited, in order to confront the evidence on the spot. We may add another remarkable incident in the case: that, after the communication of their remarks, M. Elie de Beaumont stated that, in his opinion, the gravel deposit of Moulin-Quignon did not belong to the Quaternary or Diluvian age at all, but that it was a member of the *terrains meubles* of the *actual* or *modern* period, in which he would not be the least surprised if human bones were found; adding, moreover, that he did not believe in the asserted existence of man as a contemporary of the extinct elephants, rhinoceroses, &c. of the Quaternary period! The

opinion of this very eminent and veteran geologist imports a new element of doubt into the question.

We understand that the English *savants* were received everywhere by their French opponents in the most cordial and friendly manner, and that the various questions involved were discussed in the best possible spirit. The conference lasted five days.

The *Moniteur* of Saturday last, the 16th inst., contains an article by M. Milne-Edwards, giving a brief *résumé* of the constitution and labors of the conference, and of the results to which they were conducted. It is clear that we have still much to learn regarding this very remarkable case, alike in its geological, paleontological and archeological aspects.—*Athen.*, No. 1856, May 23, 1863.

5. *The Geological evidences of the Antiquity of Man, with Remarks on Theories of the Origin of Species;* by Sir CHARLES LYELL, F.R.S. 518 pp., 8vo, with woodcuts. Philadelphia, 1863. George W. Childs. —Man is now the absorbing subject in science. Geology and zoology are bending themselves towards archeology, in order, together, to illustrate his origin and antiquity. Sir Charles Lyell presents us, in his new volume, a review of the progress which investigation has already made; and the extent to which the work has sold, both in this country and Britain, shows that in preparing it he has responded to a public demand. He reviews at length the geological developments of the few years past bearing on the subject, stating the facts with discrimination and fairness, and with all essential details. It is a work, therefore, of real value; and when science has gone forward to established conclusions, it will stand to mark a stage of progress in the important investigation.

The subject is so new that it is not reasonable to regard the work as other than an exhibition of the existing phase of this branch of science. The calculation of time from geological evidence is a mathematical problem involving many variables of unknown limits, and hence all approximations are necessarily rude. *Change of level* is one of these variables of vast influence. Geology proves it to have been in progress in all time; and even, since the Romans were in Britain, a part of Scotland, it says, has been raised 27 feet. The interior of a continent may be supposed to have changed some scores, or even hundreds, of feet in a single period, without doing violence to geological probability. This, then, is one variable affecting seriously all calculations from alluvial or delta formations. For an elevation from such a cause would increase the condensation of moisture about the heights, enlarge rivers, augment their eroding and transporting power both by increasing slope and amount of water, and thereby rapidly thicken the resulting deposits. Moreover, the same action, either in high latitudes or in low, may change, as Lyell has shown, the climate of an entire continent. This is one example of a variable, of wholly unknown limits of variation;—and one affecting calculations from coral reefs and seashore formations, as well as from alluvial beds. It is sufficient of itself to show that the future has yet much to do, before present inferences can command full confidence. Moreover, the doubts connected with the Abbeville deposits, mentioned on the preceding page, show that there are still other variables or unknown quantities to be considered.

It is quite unnecessary to give here an abstract of a volume which is made accessible to all readers, in a convenient and excellent form, through an American republication.

The later part of the volume is occupied with the Darwinian hypothesis with regard to the origin of species, which is presented with an evident leaning towards its views. The hypothesis accords with the Lyellian *uniformitarian* canon, that existing causes *under their present intensity* are sufficient to account for all past events. That caution which leads the author to hesitate before the most obvious generalizations in geology, on the ground that *all* facts are not yet known, here readily yields, and the conclusion that creation has been going on since Man as rapidly as in preceding time, and in the same way, although it has no decisive facts to support it, is accepted with favor. The above canon has done the science good service; but the whole course of geological history is against its too rigid application. We are told that judgment with regard to the transmutation hypothesis as applied to the derivation of Man from Apes should be suspended until Africa and the East Indies have been searched for the missing links. Inductive science says, on the contrary, have no faith in any hypothesis of the kind until the missing links are found. *Facts, then faith*, is the motto for science.

The proper method in geology is to present generalizations and conclusions, just as they naturally flow from known facts—using the *ignorance-argument*, not to set aside obvious deductions from the general array of facts, but to fix the limits of confidence in them; and, then, to modify these generalizations, as time moves on, wherever, and just so far as, this may be required. The uniformitarian canon, in our view, strikes in the head this method, and sets up an assumption in place of an induction.

6. *Ichnographs from the Sandstone of Connecticut River;* by JAMES DEANE, M.D. 62 pp. 4to, with 37 lithographic and 9 photographic plates.—For this beautiful volume, on the footprints of the Connecticut valley Mesozoic sandstone, the public are in a large degree indebted to the liberality and science of Thomas T. Bouvé, Esq., of Boston. Dr. Deane, as all readers of this Journal well know, was one of the earliest discoverers and most persevering investigators in this branch of American Paleontology. During the later years of his life, he was not only assiduous in making observations, but labored with great success in lithography, in order that he might make his own illustrations of his papers; and he had a work on the subject well advanced when he died. The volume, the title of which is given above, contains the plates which Dr. Deane had completed, together with the author's brief and unfinished notes, additional explanations by Mr. Bouvé, and a biographical discourse by Henry L. Bowditch, M.D. The lithographs, as well as photographs, are remarkably faithful delineations of the specimens. The footprints are referred in the work to birds, reptiles, insects, crustaceans, and worms. We are especially struck with the beauty and impressiveness of the two photographic plates of rain-prints, one, of ancient, and the other, of recent for comparison. The photographs of footprints also are excellent, and none are more interesting than those of insects and other Articulates. Eleven slabs of these minute tracks are represented on three plates.

No one can turn over the leaves of this volume without a feeling of constant regret that death should have brought the author's labors to so untimely a close; nor without a sense of gratitude towards Mr. Bouvé for the publication of the unfinished work of his friend.

7. *Notice of some new species of Fossils, from a locality of the Niagara group in Indiana, with a list of identified species from the same place;* by Prof. JAMES HALL. Published May 2, 1863. 34 pp. 8vo. Abstract read before the Albany Institute, April 29th, 1862.

8. *Die Ctenodipterinen des Devonischen Systems;* von Dr. CHRISTIAN HEINRICH PANDER. 66 pp. 4to. St. Petersburg, 1858.—*Ueber die Saurodipterinen, Dendrodonten, Glyptolepiden, und Cheirolepiden des Devonischen Systems;* von CHRISTIAN HEINRICH PANDER. 90 pp. 4to, with 17 lithographic plates in folio. St. Petersburg, 1860.—These works by Pander, on Devonian fossil fishes, are published by the Russian government, and are well worthy of the distinction thus given them. The plates are of large size, and are drawn and engraved evidently with minute accuracy. They represent the various specimens found, and also views of sections of the teeth, showing a great variety in their labyrinthine character.

9. *On the Fossil Remains of a long-tailed Bird (Archæopteryx macrurus* Ow.) *from the Lithographic Slate of Solenhofen;* by Prof. RICHARD OWEN, F.R.S.—The author details the circumstances connected with the discovery of the fossil remains, with the impressions of feathers, in the Lithographic slates of Solenhofen, of the Oxfordian or Corallian stage of the Oolitic period, and of the acquisition for the British Museum of the specimen which forms the subject of his paper.

The exposed parts of the skeleton are,—the lower portion of the furculum; part of the left os innominatum; nineteen caudal vertebræ in a consecutive series; several ribs, or portions of ribs; the two scapulæ, humeri, and antibrachial bones; parts of the carpus and metacarpus, with two unguiculate phalanges, probably belonging to the right wing; both femora and tibiæ, and the bones of the right foot; impressions of the quill-feathers radiating fan-wise from each carpus, and diverging in pairs from each side of the long and slender tail. The above parts indicate the size of the winged and feathered creature to have been about that of a rook. The several bones, with their impressions and those of the feathers, are described, and the bones are compared with their homologues in different Birds and in Pterodactyls: whence it appears that, with the exception of the caudal region of the vertebral column, and apparently of a biunguiculate manus, with a less confluent condition of the metacarpus, the preserved parts of the skeleton of the feathered animal accord with the ornithic modifications of the vertebrate skeleton. The main departure therefrom is in a part of that skeleton most subject to variety. Twenty caudal vertebræ extend from the sacrum in a consecutive and naturally articulated series, resembling in structure and proportions those of a squirrel. The tail-feathers are in pairs corresponding in number with the vertebræ, diverging therefrom at an angle of 45° backward, becoming more acute near the end, and the last pair extending nearly parallel with, and $3\frac{1}{2}$ inches beyond, the last caudal vertebra. This feathered tail is 11 inches long and $3\frac{1}{2}$ inches broad, with an obtusely

rounded end. This character of the tail is novel and unexpected because of the constancy with which all known existing and Tertiary birds have presented the short bony tail, with the terminal modification, in most of them, of the ploughshare bone.

Professor Owen next gives the results of investigations into the osteogeny of embryo birds, showing the number of vertebræ corresponding to the anterior caudals in *Archæopteryx* which coalesce with the pelvis in the course of growth, and the degree to which the posterior caudals retain a resemblance to those of *Archæopteryx* in the Birds with rudimental wings. From eighteen to twenty caudal vertebræ may be counted in the young Ostrich. In *Archæopteryx* the embryonal separation persists, with such continued growth of the individual caudal vertebræ as is commonly seen in long-tailed Vertebrates, whether Reptilian or Mammalian. The author remarks that the modification and specialization of the terminal bones of the spinal column in modern birds is closely analogous to that which converts the long, slender, many-jointed tail of the modern embryo fish into that short and deep symmetrical shape, with coalescence of terminal vertebræ into a compressed lamelliform bone, like the 'os en charrue' of birds, to which the term 'homocercal' applies—such extreme development and transformation usually passing through the heterocercal stage, at which, in Paleozoic and many Mesozoic fishes, it was arrested. Thus he discerns, in the main differential character of the Mesozoic bird, a retention of structure which is embryonal and transitory in the modern representatives of the class, and consequently a closer adhesion to the general vertebrate type.

The least equivocal parts of the present fossil declare it to be a Bird, with rare peculiarities indicative of a distinct order in that class. Although the head is absent, the author predicts, by the law of correlation, a beak-shaped mouth for the preening of the plumage; and he also infers a broad and keeled sternum in correlation with the remains of feathered organs of flight.

The paper is accompanied by drawings of the fossil and its parts, and of homologous parts in Birds and Pterodactyls. The author assigns to the fossil animal the name of *Archæopteryx macrurus.—Proceedings of Royal Society*, Nov. 20, 1862, in the *Ann. Mag. Nat. Hist.*, [3], vol. xi, No. 62.

VI. BOTANY AND ZOOLOGY.

1. *Botanical Papers in the Transactions of the St. Louis Academy of Science*, vol. ii, part 1.—Dr. C. C. Parry, in a letter to Dr. Torrey, and by him communicated to the St. Louis Academy, gives a lively and graphic account of his ascent of Pike's Peak on the first of July, 1862, with interesting botanical memoranda. Dr. Engelmann, the President of the Academy, follows with a paper on the altitude of Pike's Peak and other points in Colorado Territory, from a reduction of all the recent observations, mainly those of Dr. Parry, with an account of all the earlier *data*. The limit of trees is found at the elevation of 11,643 feet on the eastern slope of Gray's Peak, at 12,043 feet on the northern slope of Pike's Peak. The determinations of the higher summits and other interesting points will doubtless be reproduced in another part of this Journal. Dr. Engel-

mann also contributes an interesting account of the structure and morphology of *Nelumbium luteum;* a note on the nature of the pulp in the fruit of *Cacteæ*, showing that the pulp belongs to the funiculi of the seed or its appendages; also on the pulp of currants and gooseberries, which he finds "to consist of the arillus and of the modified epidermis of the testa;" a considerable paper entitled "Additions to the Cactus-Flora of the territory of the United States," mostly from the collections of Dr. Newberry (in the Colorado Expedition, 1857–58) and of Henry Engelmann in Capt. Simpson's Expedition to explore emigrant routes into Utah. On *Pinus aristata*, a new species of Pine discovered by Dr. C. C. Parry in the Alpine regions of Colorado Territory, and on some other Pines of the Rocky Mountains. *P. aristata* was probably confounded by the late Dr. James with his *P. flexilis*, but was distinguished by Dr. Parry. Both are five-leaved species, but they belong to two very different sections. They are now well cleared up by Dr. Engelmann; and, as seeds collected by Dr. Parry in considerable quantity have germinated in this country and in England, they may hereafter be familiar. Dr. Engelmann appends to this paper a revision of the character of the genera of *Abietineæ*, in a somewhat new arrangement, adopting as genera, 1. *Abies* Link. (non Linn.), the Balsam Firs; 2. *Tsuga*, Hemlock Spruce; 3. *Larix*, (these all in one group characterized mainly by the anthers); 4. *Cedrus*, Cedar, and 5. *Picea* Link, non Linn., the spruces proper, in another group; and finally, with biennial fructification, 6. *Pinus*. Dr. Engelmann also has a paper on "New Species of *Gentiana* from the Alpine regions of the Rocky Mountains," with some emendations of other species, &c. The plates, from t. 5 to t. 11, neatly illustrate *Pinus aristata*, *Gentiana Wislizeni*, *G. heterosepala*, *G. humilis*, *G. acuta*, var. and *G. prostrata*, *G. Parryi*, and *G. barbellata*, of Engelmann. A. G.

2. *The Enumeration of the Species of Plants collected by Dr. C. C. Parry and Messrs. Elihu Hall and J. P. Harbour, during the Summer and Autumn of* 1862, *on or near the Rocky Mountains in Colorado Territory*, lat. 39°–41°, is published in the Proceedings of the Academy of Natural Sciences, Philadelphia, under date of March 24, 1863. The collection, which has already been mentioned in this Journal, proves to be very interesting. A. G.

3. *Paullinia sorbilis and its products.*—Mr. Archer, in a paper read to the Botanical Society of Edinburgh, April 9, states that from the large seeds of this Sapindaceous plant "is manufactured the substance called *Guarana*, which is extensively used in Brazil, Guatemala, Costa Rica and other parts of South America, as a nervous stimulant and restorative. The seeds, deprived of their covering, are pounded into a paste, which hardened in the sun constitutes *Guarana*. It is used both as a remedy for various diseases, and also as a material for making a most refreshing beverage. . . . . The presence of an alkaloid, which he called *Guaranine*, was discovered some years ago in Guarana by Dr. Theodore von Martius of Erlangen; but its identity with Theine was soon established; and subsequent analyses, especially one by Dr. Stenhouse in 1856, proved that not only was the active principle of Guarana identical with Theine, but that, as far as known, no other substance yields it so abundantly, —the amount being 5·07 per cent, as against good black Tea, which

yields 2·13, and Coffee, from 0·8 to 1·00. The mode of using the Guarana is curious and interesting. It is carried in the pocket of almost every traveller, and with it the palate bone or a scale of the large fish, *Sudis gigas*, locally called 'pirarucu,' the rough surfaces of which form a rasp upon which the Guarana is grated; a few grains of the powder so formed are added to water, and drunk as a substitute for tea. The effect is very agreeable, but, as there is a large quantity of tannic acid also present, it is not a good thing for weak digestions."—*Extr. Gard. Chron.*, May 2, p. 414.

4. *Aerial rootlets on the stems of Virginia Creeper* (*Ampelopsis quinquefolia*).—Referring to the note on p. 445 of the last No. of this Journal, we may add that we have received, from Dr. Parry of Iowa, specimens of the stem of this plant abundantly garnished with aerial rootlets, thus confirming the character in Michaux's *Flora*, and in the lamented Dr. Darlington's *Flora Cestrica*. A. G.

5. *Martius, Flora Brasiliensis*, fasc. xxxi, xxxii. (Jan., 1863).—These new parts of this great work contain the *Dilleniaceæ*, by Dr. Eichler, with 14 plates; and the *Sapoteæ*, by Prof. Miquel, 31 plates. Both orders appear to be well elaborated. A. G.

6. *Dr. Charles Wilkins Short* died at his residence at Louisville, Kentucky, on the 7th of March last, in the 69th year of his age.[1] He was born in Woodford County of that State, on the 6th of October, 1794, was educated in the school of Mr. Joshua Fry, near Danville,—a distinguished teacher of those days,—pursued his professional studies mainly in Philadelphia, where he took the degree of M.D. from the University of Pennsylvania in the year 1815. For ten years he devoted himself to the practice of medicine, until, in the year 1825, he was called to the chair of Materia Medica and Medical Botany in the Transylvania University at Lexington, where he contributed to the reputation of that celebrated school. Relinquishing medical practice, for which he had no liking, he devoted his powers with zeal and success to the more congenial duties of his professorship, and to the cultivation of botany, the favorite pursuit of his life. At the close of the year 1838, he removed, along with some of his distinguished colleagues, to Louisville, filling the same chair in the University of that city, until 1849, when he retired from public functions. The remainder of his honorable life was passed at Hayfield, his tasteful residence near Louisville, in the bosom of his family; in the exercise of kindly but unostentatious hospitality and of all good offices; in quietly enjoying and causing others to enjoy the blessings of a handsome fortune, to which by inheritance, combined with the fruits of his own industry, he had now attained, and in the cultivation of "the amiable science" to which he was devotedly attached.

Dr. Short's botanical publications were neither large nor many. They were chiefly articles contributed to the Transylvania Journal of Medicine, &c., of which he was for some time one of the editors. The most important is his Catalogue of the plants of his native State (which he widely and assiduously explored), and several Supplements; with well considered characters of some new species, and acute and discriminating notes upon several imperfectly known plants. These, and the copious MSS. observations which he was accustomed for many years to commu-

[1] See this Journ., xxxv, 451.

nicate to his botanical correspondents, showed what he was capable of accomplishing, had not a most retiring and unambitious disposition unduly limited his exertion. It was not activity or persevering labor, but publicity, that he shrunk from. He was a very industrious botanist, and an effectual promoter of our science in this country. His great usefulness in this field was mainly owing to the extent and the particular excellence of his personal collections, and to the generous profusion with which he distributed them far and wide among his fellow-laborers in this and other lands. He and the late Mr. Oakes—the one in the west and the other in the east, but independently—were the first in this country to prepare on an ample scale dried specimens of uniform and superlative excellence and beauty, and in lavish abundance for the purpose of supplying all who could need them. Dr. Short's disinterested activity in these respects has enriched almost every considerable herbarium both at home and abroad, and set an example which has produced large and good results among us. The vast improvement in the character of the dried specimens now generally made by our botanists may be mainly traced to the example and influence of Dr. Short and Mr. Oakes. As might be expected, Dr. Short's own herbarium is a model of taste and neatness. It is also large and important. To one himself so solicitous "to do good and to communicate," contributions from numerous sources naturally flowed in abundantly. He, moreover, subscribed to all the North American distributed collections within his reach, and he set on foot or efficiently furthered several distant or difficult botanical explorations. He purchased, at a liberal price, the important botanical collections of Texas and Northern Mexico, left by Berlandier, which Lieut. (now General) Couch acquired of his widow, and sent on to Washington; and, retaining one set for his own herbarium, he caused the rest to be distributed among the botanists to whom they would be most useful,—especially including two Swiss botanists who had contributed to send out Berlandier to Mexico as a collector, but from whom (apparently through Berlandier's dishonesty,) they had failed to receive any adequate return. It is understood that Dr. Short's rich herbarium—to which a lifetime of thoughtful attention and much expense were lovingly devoted—is now offered, by a wise bequest, to the custody of the Smithsonian Institution, under instructions that it shall be permanently well cared for and always open to be consulted by botanists. It will there form an excellent and conspicuous nucleus for a collection of American herbaria, such as our science needs, and the country ought to possess.

The natural effects upon his scientific career of a fastidious taste, an unwarrantable diffidence, and a too retiring disposition, were enhanced by a constitutional tendency to depression of spirits. But this never obscured the native kindness of his heart, and the real, though so quiet, geniality of his disposition, or checked an unobtrusive and considerate benevolence. With an uncompromising sense of right and justice, and a keen hatred of everything mean and unworthy, he was never harsh nor even cynical. All who knew him well, and also his more intimate correspondents who never enjoyed the privilege of a personal acquaintance, can testify to the nobility and Christian excellence of his character. An appreciative tribute to his memory, from the pen of a former colleague,

will be found in the *Louisville Journal*, issued a few days after Dr. Short's lamented death.

Two or three species of Kentucky plants commemorate the name of Dr. Short as their discoverer. Also a new genus, *Shortia*, inhabiting the Alleghany Mountains, was dedicated to him by the present writer. But, alas! too like the botanist for whom it was named, it is so retiring in its habits that it is not known as it ought to be, but lives as yet unseen, except by a single botanist of a former generation, in some secluded recess of the Black Mountain of North Carolina. It will some day be found again and appreciated. A. G.

7. *The late Wm. Darlington.*—Not unexpectedly are we called to add to the list of the departed, the name of this venerable and excellent man. The Nestor of American Botanists died, at his residence in West Chester, Penn., on the 23rd of April last, having nearly completed the 81st year of his age. The death of this most charming and unaffectedly good man, although occurring in the fulness of time, and following close (as was meet) upon the bodily infirmities which at length arrested the serene activity of the octagenarian,—waiting, but laboring still as strength and occasion served,—is sensibly felt, not only in the town and county where he has long been honored and venerated, but also by a wide circle of friends and correspondents throughout the country and in other lands. If not a very profound, he was a most accurate and faithful botanist, one who appreciated and largely imbibed the spirit of all the great advances in botanical philosophy, and especially in morphology, which have been made in his day. His *forte* was in the clear and accurate description of plants; his desire, to make perfectly known the plants of his native county; his modest estimate of his own labors in his favorite pursuit was expressed in the motto of his *Florula Cestrica*, in 1826, and repeated in his classical *Flora Cestrica* (one of the very best local floras ever written) in 1837 and 1853: "*Ore trahit quodcunque potest, atque addit acervo:*" his love for the familiar objects which had attracted his life-long interest was characteristically shown in the inscription which he wrote for the stone that now covers his mortal remains: "*Plantæ Cestrienses quas dilexit atque illustravit super tumulum ejus semper floreant;*" while higher feelings and sacred hopes were fittingly expressed when on receiving the first warning, of which he knew well the significance, he said to those around him, "My work is done; and I think I can say, with Simeon of old, 'Lord, now lettest thou thy servant depart in peace.'"

But, much as he cultivated Botany, this was only the side-issue, the recreation of his life, which was actively devoted to professional and various civic occupations, and to the discharge of many honorable trusts. Such biographical notices as may well be added here, we will select from a "Memorial of Wm. Darlington, M.D.", drawn up by one of his townsmen, of which a copy has just been received by us.

"He was born near the ancient village of *Dilworth*, now called Dilworthstown, in Birmingham township, Chester county, Pennsylvania, April 28, 1782.

"His great grandfather, Abraham Darlington, the son of Job and Mary Darlington of Darnhall, in Cheshire, England, came, while a young man, with his brother John, to Pennsylvania, in the beginning of the last century, and settled at first near Chester. * *

"The grandfather of William Darlington was a farmer, and resided in East Bradford township, Chester county. He married Hannah, a daughter of Edward Brinton, a member of an old and respectable family that had come over to America amongst the earlier settlers of Pennsylvania. He raised and educated nine sons and two daughters, and died in the Autumn of 1808.

"Edward Darlington, the eldest son of Thomas, and father of William, was educated a farmer by his maternal grandfather, from whom he received, by will, the farm in Birmingham township, on which he was reared, and which is now in the possession of his grandchildren.

"He married Hannah, a daughter of John Townsend, of East Bradford, Chester county, by whom he had five sons and two daughters. He was an intelligent man, self-educated, and exercised a considerable influence amongst the citizens of his county, by whom he was several times elected a member of the State Legislature. He died in 1825.

"William Darlington, of whom we shall now speak, was the eldest child of Edward and Hannah Darlington, and descended from ancestors, each branch of which, as far as it can be traced, was an unmixed race of plain English Quakers. He was early inured to the severe labors of agricultural life, and, when old enough to drive or hold the plough, was kept at work in the summer, and only permitted to go to school in the winter season. The common country schools of that day were lamentably deficient as compared with those of modern times, yet he succeeded in obtaining a plain English education, under John Forsythe, an Irish Friend, one of the best teachers of that time in the county, and who, during a long period spent in that vocation, imparted the rudiments of education to many who have since become eminent and useful citizens of the republic.

"Becoming tired and disgusted with the drudgery of farm labor, which then was not one tithe as attractive as it has since been made by the inducements offered and efforts made by Agricultural Chemistry, and Agricultural and Horticultural Societies, and the improvement in machinery whereby so much of the labor of the farm is now avoided, William, after much difficulty, induced his father to permit him to study medicine. With this view, in the Spring of 1800, he entered the office of Dr. John Vaughan, a respectable physician of Wilmington, in the State of Delaware.

"Whilst pursuing with assiduity the study of that profession which he had selected as the business of his life, he devoted those hours which, with many, would have been given to idle recreation, to acquiring a knowledge of the French language under a private teacher, and there developed a passion for the study of languages which remained with him for life, and enabled him subsequently to make an excellent and satisfactory acquaintance with the French, Latin, Spanish and German, when opportunity was afforded. So strong was his taste for such acquisitions, that at the age of fifty, a period when many men think their labors are over, he embarked in the study of the noble Castilian tongue with all the ardor of a schoolboy of seventeen, and mastered it thoroughly.

"In 1802 the malignant Yellow Fever was fearfully prevalent in many places throughout the Union, and scourged the country with a violence that made the boldest physicians shrink aghast from its awful ravages. Amongst other places, it visited Wilmington, carrying terror and desolation in its train. Large numbers of the citizens sought safety in flight; even *physicians* left the place, and the only medical personages that remained were Dr. Vaughan and his pupil William Darlington,—who with great moral courage faithfully continued at their posts, and rendered their services to those afflicted with the fearful epidemic.

"In the Winters of 1802–3 and 1803–4, William Darlington attended the medical lectures in the University of Pennsylvania, and on the sixth of June, 1804, he received the degree of Doctor of Medicine, being, as the writer

believes, the first citizen of Chester county who took that degree in that University. For a long term of years, and until he relinquished the duties of his profession, he was confessedly the head of that profession in the county of his birth. The subject of his inaugural Thesis was "the mutual influence of habits and disease," an essay which, from the soundness of its views and depth of scientific research, received a flattering compliment from Professor Rush, at a public examination on the day prior to the commencement.

"Whilst preparing his Thesis, after the close of his second course of medical lectures, Dr. Darlington attended the botanical lectures of Professor Benjamin Smith Barton, and thus began his first acquaintance with that science whose beauties and pleasures he has, in later years, done so much to illustrate, and in so successful a manner as to make his name known and respected throughout the botanical world.

"On receiving his diploma, he returned to his native place and commenced the practice of medicine, and in his leisure hours availed himself of the first opportunity that presented, of making himself familiar with the Latin language, which in those days seemed to hold the key of the temple of the physical and natural sciences. In the following year, he was appointed physician to the Chester County Alms House, and also surgeon to a regiment of militia. The latter appointment, however, caused his disownment by the Society of Friends, of which he was a member, as it was contrary to their discipline to assist in, or encourage, war, in any manner whatever.

"Since that day, however, the views of the 'Friends' seem to have changed somewhat, upon this subject, and the former rigidity of the discipline in regard to it has relaxed, and there are now in the Union armies large numbers of young Friends, who are offering their lives in the service of their country, in as earnest and effectual a manner, and with as unselfish a patriotism, as the men of any other religious denomination.

"In 1806, Dr. Darlington received the appointment of surgeon to an East India Merchantman, belonging to Philadelphia, and made a voyage to Calcutta,[1] whence he returned the following year. He availed himself of the leisure afforded him in the long voyage, to make an acquaintance with some of the best works then extant in English literature. A sketch of the observations made during this voyage was, some years afterwards, published in the form of familiar letters in the Analectic Magazine.

"In the year succeeding his return from Calcutta, he settled in West Chester, and resumed the practice of medicine, and was soon in the enjoyment of an extensive and profitable business, which embraced a large extent of country, and required laborious industry and perseverance to give it the requisite attention, as physicians then were few and far between, in the rural districts. The famous embargo, in Jefferson's administration, prevented any further voyaging by sea, but circumstances had occurred in the meantime which would have detained him at home without an act of Congress; for, on the first of June of that year (1808), he was married to Catharine, daughter of General John Lacey, of New Jersey, an officer who had served with credit and ability in the revolutionary war.

"Always anxious for self-improvement, Doctor Darlington commenced the German language about that time under a private tutor, and soon made himself sufficiently familiar with it to be enabled to enter into the spirit and enjoy the beauties of the great writers in that tongue. The love of the German then acquired increased with years, and at the ripe age of 81, and up to the hour of his death, he enjoyed the immortal works of Schiller, Lessing, Goethe, and other German authors, with which his library was stored, with all the zest which the strength of diction, harmony of verse, and beauty of thought, that characterize the writings of those eminent men, are so well calculated to

[1 Where he made the congenial acquaintance of the late Dr. Wallich, the well known botanist, and director of the Calcutta Botanic Garden.—Eds.]

inspire. He was fortunate, too, in having instilled into one of his daughters the same love of language which imbued his own mind, and her familiar knowledge of the Latin, French, and German tongues enabled her to add to his happiness in his later years, by sharing with him those pleasures which frequent converse with the best authors in those languages never fails to ensure.

"When the war with England broke out in 1812, the subject of this sketch, with other young men of the neighborhood, offered their services in defense of the altars and firesides of their country in case of invasion. A volunteer company was formed and drilled at West Chester, ready to serve when called upon, and in September, 1814, on a requisition by the Governor of Pennsylvania for volunteer troops to aid in the protection of Philadelphia, which was supposed to be threatened by the enemy then in Chesapeake Bay, he went to the camp on the banks of the Delaware as an ensign in the "American Grays." Having some taste and skill in military tactics, the regiment into which his company was incorporated chose him Major of the first battalion. In this post he served until the corps was disbanded, and was rewarded like his fellow-soldiers with the meagre pay of that day, and the still more meagre national grant of forty acres of the public domain.

"In the meantime, however, his fellow citizens at home, appreciating his worth as a physician, a friend of education, a citizen soldier, and an enlightened statesman, elected him, unsolicited, a member of the 14th Congress. In 1816, in consequence of dissatisfaction existing towards his colleague in another county, (the single district system not having been then adopted,) he lost his election by the small majority of seven votes, but this defeat was amply atoned for by triumphant elections to the 16th and 17th Congress, from the same district.

"During his second term the celebrated Missouri question agitated the Union from one end to the other, and called forth the ablest efforts of the best men in Congress. On that question Dr. Darlington was found ranked with those who were desirous to restrict slavery, and raised his voice in an able and excellent speech in opposition to its extension.

"On that occasion he said: 'We are solemnly bound not only to secure our own welfare, but to provide, as far as we can, for that of our posterity. When we *know* that the welfare of our descendants in Missouri, as well as in the United States generally, *requires the restriction of slavery*, how can we reconcile it to our sense of duty to permit the unnecessary introduction and diffusion of an evil which we are sure *will be the scourge of countless generations*.'" * * * *

"Dr. Darlington was one of the members of the first board of Canal Commissioners, and was associated with such men as Albert Gallatin, John Sergeant, Robert W. Patterson, and David Scott, whose names hold a distinguished place in our country's annals. He served in that station two years, during the last of which he was President of the board."

"The duties alluded to, however, though arduous and exacting, did not prevent Dr. Darlington from bestowing some attention to Natural Science, and indulging his taste for botany. In 1826, in conjunction with some of his intimate friends, he assisted in organizing the Chester County Cabinet of Natural Science, of which institution he was President from its origin; and in the same year he published his 'Florula Cestrica,' being a catalogue of plants growing around the borough of West Chester, Pennsylvania.

"The arduous duties of the office of Canal Commissioner, being then performed gratuitously, and calling him away from home more than was either convenient or agreeable, he resigned that office the next year, and was almost immediately thereafter appointed Prothonotary and Clerk of the Courts of his native county, by his political and personal friend, the late lamented Governor Schulze, the duties of which office he continued to discharge till 1830.

"Whilst in the office of Prothonotary Dr. Darlington and some of his medical friends co-operated, and formed the Medical Society of Chester county, an institution which has had the good effect of uniting in a fraternal union almost all the physicians of the county. Through its periodical meetings, addresses, written communications, and debates, it has been the means of promoting the increase of medical knowledge, of establishing an *esprit de corps* amongst medical men, and of removing those petty jealousies which are too apt to arise in a profession whose country members live in comparative isolation, and have very little communication with one another. From his long standing in his profession, and the skill which he had acquired by an extensive practice, Dr. Darlington was unanimously placed at the head of the Society, which position he held till 1852, when he resigned and was immediately elected an honorary member."

"About the same time he assisted in exploring a route for a railroad from West Chester towards Philadelphia to intersect the Columbia railroad.

"Through the exertions of himself and a few gentlemen of West Chester, a company was formed, of which he was made the first President, and superintended the construction of the road, which was the first private tributary to the line of public works."

"In 1830, he was elected president of the Bank of Chester County, of which institution he had been one of the commissioners named in the Charter for receiving subscriptions of its capital stock, and a director almost ever since its establishment in 1814. He was re-elected annually, and continued in that station to the time of his death."

"Its currency was so regulated, and its discounts so discreetly made, that it still continued to be an instrument of good to the citizens of the county in which it was located. It possessed their entire confidence, and its notes were eagerly sought after in preference to those of most other banks within the range of its circulation. These happy results were mainly due to the financial abilities of the president and his old and long tried friend, David Townsend, late cashier of the bank, a gentleman who, it is not improper to state here, was associated with Dr. Darlington in nearly all of the public enterprises of a local character in which the latter was engaged. His acquirements in the Doctor's favorite science of botany, together with the excellence and value of his exchanges of plants with European botanists, obtained for him the high compliment of having his name conferred upon a new and interesting genus of Arctic American and Rocky Mountain plants, by his friend Professor Hooker, the learned and talented Director of the Royal Botanical Gardens at Kew, near London.

"A similar honor was conferred on Dr. Darlington in 1825, by Professor DeCandolle, of Geneva, for his eminent services in the beautiful science. The genus dedicated to him by DeCandolle did not, however, prove to be sufficiently distinct to maintain its place as an independent genus, and his friend Professor Torrey, of New York, dedicated to him a new and splendid genus of California Plants, of the natural family of Sarraceniaceæ, which, from its rarity and beauty, constitutes a worthy and fitting compliment to an industrious laborer in the agreeable fields of botanical science.

"To his botanical friends it may be interesting to learn, that Professor Gray, of Cambridge, has just succeeded in raising it on the Atlantic slope, and we may soon have the pleasure of cultivating in our gardens the beautiful Darlingtonia.

"It is too seldom that we find a love for natural science, or the fine arts, in a temple devoted to mammon. The Bank of Chester County, however, is an exception. The President and Cashier of that institution prosecuted their scientific researches together, collecting treasures for their herbaria o'er hill and valley, and making exchanges with many of the most eminent botanists of Europe, whilst the present cashier, (Mr. William W. Jefferis,) is an excel-

lent mineralogist, and has collected one of the best mineral cabinets in the State, and furnished many valuable contributions to mineralogy, in the discovery of new and heretofore undescribed species and varieties.

"In the year 1837, Dr. Darlington published his 'Flora Cestrica,' a description of the flowering plants of Chester county, which was a new edition of his former work, much enlarged and greatly improved.

"This work is regarded as one of the most complete local Floras extant, and is a model for all works of a similar character that may be constructed on the artificial system of botany.

"Conceiving the idea of assisting the farmers of our country by a work expressly devoted to an account of those plants which it more especially concerns them to know, he prepared and published in 1847 his 'Agricultural Botany,' in which are described in plain and familiar terms not only the useful *cultivated* plants, but all those which a careful and industrious farmer should extirpate from his soil. This work is one of the practical benefits which natural science sometimes bestows upon mankind, and there is good reason to believe that its influence has already produced a beneficial effect upon husbandry, not only in Chester county but elsewhere.

"The deep interest he always felt in every votary of natural science, together with a strong personal attachment for a friend, induced him at an earlier day, (about 1843,) to collect together the letters, memoranda, &c., of Dr. William Baldwin, a native of his own county, who was also passionately devoted to botany, but who died at an early age while on the expedition up the Missouri, under Major Long. These remains were given to the world in a volume entitled 'Reliquiæ Baldwinianæ.'

"The pioneers of botany in Pennsylvania were Humphrey Marshall and John Bartram; the former residing near West Chester, the latter at Darby, near Philadelphia. Dr. Darlington collected, in 1849, such portions of their correspondence as still remained in existence, comprising, together with their own letters, those of many eminent botanists of the day, and published them in one large volume, with illustrations of their homes, under the title of 'Memorials of Bartram and Marshall.'

"This correspondence of our earlier botanists affords a pleasant insight into their scientific labors, and shows the dangers they underwent, and the difficulties they had to encounter in the early settlement of the country, during their expeditions into the wilderness in the prosecution of their favorite science. The former home of Humphrey Marshall still stands at Marshalton, in Chester county, and the rare and curious forest trees that he planted with his own hands around it have grown with years, until they have become objects of great interest to every votary of botanical learning.

"Dr. Darlington's latest labors in the cause of natural science consist in a new edition of the 'Flora Cestrica,' revised and reconstructed on the natural method, which seems to be the system most generally adopted by scientific botanists at the present day. Besides this, in connection with some of the liberal minded men of his neighborhood, he was engaged in his latter years in the composition of a work descriptive of the objects of the Natural History of Chester County in all its branches.

"In the Spring of 1862, he was attacked by a slight stroke of paralysis, from which he partially recovered, but with some prostration of his physical vigor. This was followed in the early part of 1863 by another attack of the same disease, from the effects of which he gradually sank, until on Thursday, the 23d of April, 1863, aged nearly 81 years, he went to his final rest, with his mental vigor unimpaired, having evinced in his conversation with his children, during his last illness, the same love of science and literature which had characterized him through a life protracted much beyond the usual period allotted to man."

"In order that all the people of the county, who desire improvement in natural science, may continue to have, after his death, the same sources of knowledge as he could afford them in his life, he has bequeathed his most valuable herbarium of plants, and all his botanical and most of his other scientific works to the Chester County Cabinet of Natural Science, on whose shelves they are designed to remain as a rich mine from which the earnest students of nature may always be enabled to glean most precious fruits.

"He enjoyed, in an eminent degree, the friendship of the best botanists of his day, and his correspondence with the distinguished DeCandolle, and Sir William Jackson Hooker, of the old world, and Doctors Torrey and Gray of the new, attest the high value they placed on his contributions to the gentle science of which he was so fond, and which, with them he assisted so much to illustrate.

"In his social relations he was the kind friend, whose heart and purse were ever open to assist struggling merit, in whatever walk of life it might be found, and his contributions to all purposes of benevolence, philanthopy, or knowledge, were, according to his means, of the most generous character. He was an indulgent parent, whose earnest desire was to make his family useful to themselves and the community, in which he has happily succeeded, and he was the pleasant neighbor, whose extensive knowledge, excellent memory, and agreeable conversational powers, made him a most interesting companion.

"His mind was, through his whole life, ever prompt and active, and in the last work of his hands, 'Notæ Cestrienses,' or sketches of the most distinguished men of his county, which was undertaken with his friend, J. Smith Futhey, when he was nearly eighty years of age, and which was finished only a few months before his death, he felt that each biographical sketch might be the last, and he labored upon it with youthful zeal and earnestness, that it might be finished before he heard the Master's call.

"He died as he lived, a christian gentleman, of great purity and simplicity of character, whose whole life was never stained by a mean, ungenerous, or dishonest action.

"From this slight sketch of Dr. Darlington, it will be observed that he has been a man of both thought and action, of books and deeds, and has spent a busy life in the service of his county, his state and nation, and endeavored, in a quiet and unostentatious manner, to disseminate information amongst the masses of the people.

"Although greatly esteemed for his literary abilities, which have been highly self-cultivated, yet his strongest hold on the public regard arose from the earnest efforts he has been ever ready to make in the cause of natural science and popular education, from the time, when, like the widow of old, he gave his mite, being all he was then worth, towards the good cause, down to the last years of his life, of which he devoted the hours most men give to rest, to the agreeable and useful task of diffusing knowledge among men.

"It is pleasant to know that those labors have been properly appreciated by men whose commendations are of value, as may be found in the fact that the self-taught farmer's lad has had his name and fame bequeathed to future time so long as plants shall grow and bloom; that he received in 1848 the highly honorable degree of LL.D., from Yale College, and was elected a member of more than forty literary and scientific associations.

"The West Chester Academy, the Medical Society of this county, the Chester County Cabinet of Natural Science, the West Chester Library, the Agricultural and Horticultural Societies of Chester county, were all indebted to him for his valuable aid in giving the popular impulses which brought them into existence, and which have since continued and extended their usefulness.

"His example and instructions, his books and pamphlets, his discourses and lectures, infused a taste for literary and scientific information into the minds

of the young around him. * * * They have been the means of awakening a thirst of knowledge amongst the people of the place of his residence, and a desire for good educational institutions, until Chester county has become noted for the general intelligence of its citizens, and the excellence of its numerous schools.

"Temperate in his habits, moral and religious in his character, in the full maturity of years, and with his mental faculties almost unimpaired to the last, he enjoyed with satisfaction, at a good old age, the consciousness of a life well spent, and the contemplation of the ripened fruits produced as the results of his earlier and later labors, and in the enjoyment of the respect of a grateful community, he was enabled to feel that his career had been a useful one to the people amongst whom Providence had placed him, and that his years were not employed like those of the fool or the sluggard, but improved to the benefit of himself and of future generations, as the seeds of knowledge once sown are not for a single harvest, but for all future time."

It is understood that Dr. Darlington has left in the hands of a friend an autobiography. We know not whether this was written in view of future publication; but it may probably with propriety be printed, after some lapse of time, along with selections from his correspondence, for the gratification of the numerous friends of the writer, or even for the instruction of a wider circle of readers. A. G.

Zoology—

1. *Observations on the genus Unio, together with descriptions of new species, their soft parts and embryonic forms, in the family Unionidæ, and descriptions of new genera and species of the Melanidæ.* By Isaac Lea, LL.D., President Acad. Nat. Sci., &c. vol. ix. With sixteen plates. Read before the Academy of Natural Sciences of Philadelphia, and published in its Journal.—We have again the pleasure of noticing a volume of Mr. Lea's works, which is perhaps superior in value and research to any of his former ones. This memoir forms the greater part of vol. v, of the new series of the Journal of the Academy of Natural Sciences, and is accompanied with sixteen quarto plates most admirably executed, and constitutes vol. ix of "*Observations on the genus Unio*, &c." The Unionidæ embrace descriptions of 28 new species, illustrated by ten plates, giving three views of each species. The remaining six plates embrace 229 new species of Melanidæ, the figures executed on stone with remarkable beauty and correctness. For the greater part of these Melanidæ Mr. Lea has proposed three new genera, *Trypanostoma*, *Goniobasis*, and *Strephobasis*, considering that they do not belong to the genus *Melania*, as described by Cuvier and Lamarck. Mr. Lea has entered into a full discussion of these new genera in the work before us, and he finds them sustained by the anatomy of the soft parts. He also gives the following brief analysis of the genera of Melanidæ.

Melania having a regular loop-form aperture.

Anculosa having a rounded aperture and a callous columella.

Io having a greater or lesser elongate channel or spout at the base.

Lithasia having a callus on the columella above and below, and a notch at the base.

Schizostoma having a cut in the upper part of the outer lip.

Strephobasis having a retrorse callus at base, and usually a nearly square aperture.

Trypanostoma having an expanded outer lip, and an auger-shaped aperture.

Goniobasis having usually a sub-rhomboidal aperture, sub-angular at base and without a channel.

Amnicola having a round mouth and no callus.

The number of new species of Unionidæ which Mr. Lea has described, since he commenced the study of this family in 1827, already amounts to over 550, nearly all of which are indigenous. Full descriptions, accompanied by excellent figures, have been given of each, and, when the soft parts could be obtained, descriptions of these have been added, and, to some extent, drawings of their anatomy. A great advance has been made by describing and illustrating the embryonic forms, by which many of the species are shown to differ more than in the adult or mature state. This is a branch of study which few European Malacologists have attempted.

According to Mr. Lea, we have now, in the United States, 633 known species of the family Unionidæ, divided as follows: *Unio* 542, *Margaritana* 31, *Anodonta* 60. To these may be added, for the remaining part of North America, as far as yet known, 41 species, making together 674 species of this family. Mr. Lea has many other new species undescribed, and vol. x of "Observations" is in an advanced stage of preparation.

The figures of the Melanidæ are among the best figures of shells ever published, and they do credit to our American artists. Alabama seems to be the geographical center of this family, or the region having the greatest number of species and individuals. Other new genera yet may be required; and when so, it is important that they should be based, as far as possible, on characters derived from the structure of the animals, all others being liable to lead to error. Only careful study can teach what modifications of form depend upon fundamental points in the animal structure. We are pleased to see dimensions given with so much care: this shortens the labors of those looking up species, especially where there are no figures. Mr. Lea would do a great service to science by preparing a paper on the species found in all the great river basins, both Unionidæ and Melanidæ, with a view to the zoological regions.

It has been stated by a recent writer in Paris, in noticing the large number of species described by Mr. Lea, that probably not a tenth of them would prove to be distinct. It may well surprise a European naturalist that the species should be so numerous in America. But the remark is a betrayal of ignorance of the subject, and also of those hygrometric conditions of the continents of Europe and America in which they so widely differ, and which are the basis of great zoological distinctions. A view of the finely executed figures should alone satisfy a conchologist of judgment, that the various forms, so remarkable in our Western and Southern rivers, could not fail to prove to be, with few exceptions, at least, distinct species. It is quite possible that there have been some duplications, for this could scarcely be otherwise where specimens are rare; and especially in the earlier descriptions, before the difference of form in the two sexes, a fact unappreciated by European naturalists, had been observed.

Science is greatly indebted to Mr. Lea, for his untiring labors through so many years in this department of Natural History. H.

2. *Researches upon the Anatomy and Physiology of Respiration in the Chelonia;* by S. Weir Mitchill, M.D., and George R. Morehouse, M.D. (*Smithsonian Contributions to Knowledge*, xiii, 169.)—This memoir is one of great interest, not only on account of the admirable manner in which the results have been worked out, and the valuable contribution which they are to physiology, but as showing how the labors of a careful and truthful observer may pass for more than a half of a century neglected, until, falling under the notice of those who know how to appreciate them, the place which is their due is claimed for them in the history of science.

The authors have made a thorough demonstration of the manner in which the respiratory movements are executed in turtles, and have shown that, with one exception, all writers on the subject from Malpighi to Agassiz, including no less authorities than Cuvier, Johannes Muller, and Milne-Edwards, have fallen into error. The mechanism of breathing in these animals, as described by naturalists, has been supposed to be as follows: by the depression of the hyoid apparatus and tongue, air is drawn into the mouth through the nostrils; these are then closed, and, by the raising of the hyoid, air is driven from the mouth through the glottis and trachea into the lungs, when inspiration is completed; expiration is effected by the contraction of the abdominal muscles, and the consequent compression of the lungs. This is all wrong.

The authors of the memoir have proved that the hyoid apparatus has nothing whatever to do with ordinary breathing, and that the movements of respiration are really effected in the following manner: inspiration by the abdominal muscles, which, when in repose, form a deep concavity in either flank behind the sternum, those on each side somewhat resembling a diaphragm in shape; as they contract, they become flattened, descend, draw down the viscera, and in so doing enlarge the cavity of the trunk, which enlargement is followed by a rush of air through the trachea into the lungs, when inspiration is completed. Expiration is produced by the action of a peculiar muscle, which the authors have for the first time completely described, and which previous writers have for the most part overlooked, or have attached but little or no importance to it. This has by some been doubtfully considered as the homologue of the diaphragm, and may be described as a broad digastric muscle, one of the bellies arising from nearly the whole breadth of the fore, the other from the hind part of the shield and from the pelvis, and the two united by a broad tendon across the middle of the abdominal cavity; between this muscle in front and the shield behind are included the viscera. By the contraction of it, and the consequent compression of the lungs, the movement of expiration is effected. We have taken pains to test this explanation on a living animal, and have constantly found that the flanks descended and were flattened during inspiration, and ascended and became more concave during expiration, the hyoid apparatus all the while being motionless. Thus the comparison which, since the days of Malpighi, has been made between the respiration of frogs and turtles, proves to be unfounded,—and the abdominal muscles, which in other air-breathing vertebrates are expiratory, become inspiratory in the turtles, while the presumed homologue of the diaphragm is the true muscle of expiration.

The authors had reached the conclusion set forth in their memoir, when, on looking into the literature of the subject, they found that in some respects they had been anticipated, as appears by reference to a *Dissertation on the Respiration of a Tortoise, by Robert Townson, LL.D.*, written at Gottingen, May, 1795. This the authors have reprinted in full, "as an act of justice; for, having conducted our enquiry with a full knowledge of the opinion of modern authorities, we were surprised, on afterwards learning the singularly truthful views of Townson, to find that they had fallen unappreciated, and that in many instances they had not even been honored by a notice, or when noticed had been mentioned only to be condemned."

In the dissertation above referred to, Townson fairly demonstrated the action of the abdominal muscles, and fairly recognized their true use, his conclusions being based upon a series of well devised experiments. He however had but an imperfect knowledge of the compressor muscle, being ignorant of the anterior portion of it, and supposing that pressure was applied only to the hinder lobes of the lungs instead of the whole of them. Townson's explanation, which has been not only for the most part neglected, but criticised by Cuvier as erroneous, has at length been raised from obscurity, and proved, as far as it goes, in all respects true, and will hereafter receive the credit which it so richly deserves.

In addition to the above mentioned results, the memoir contains a very important, and, so far as we are informed, an entirely new, investigation of the physiology of the nerves governing the movements of the glottis. We should pass beyond the limits of a merely bibliographical notice if we entered upon the anatomical and experimental details on which it rests, and will simply add the following summary of them by the authors, so far as they relate to the nerves of the larynx.

1st. In Chelonians, the superior laryngeal nerve is distributed both to the opening and closing muscles of the glottis.

2d. The inferior laryngeal nerve is distributed solely to the opening muscle of the glottis.

3d. A true chiasm exists between the two superior laryngeal nerves.

This last proposition covers a curious and hitherto undescribed distribution of the laryngeal nerves, viz: a complete decussation or intermingling, by means of the chiasm, of filaments from opposite sides, in a manner analogous to those of the optic nerve; so that the muscles of the right half of the larynx not only get fibres of the superior laryngeal nerve of the same side, but, as it appears, in about an equal proportion from the opposite nerve. The results obtained in experimenting upon this nerve will be readily explained by the anatomical facts just mentioned. After dividing the lower or recurrent laryngeal nerves, the motions of the glottis continue, since the upper nerve supplies both opening and closing muscles; if in addition the upper nerve of one side is divided, they still continue, because filaments of the undivided nerve not only pass to the muscles of the same side, but, by means of the chiasm, to those of the opposite; if the upper nerves of both sides are divided, the paralysis is complete.

The memoir contains a large number of anatomical details and of physiological experiments of great interest, which we must leave unno-

ticed: we will only add that the investigations have been conducted throughout with great care and skill, showing that the authors are adepts in the only school which is likely to enlarge the boundaries of physiological knowledge, viz: the school of observation and experiment.

J. W.

V. ASTRONOMY AND METEOROLOGY.

1. *Discovery of Asteroid* (78).—This planet was discovered by Dr. R. Luther, at Bilk, March 15, 1863. It appeared as a star of the 10th magnitude, and has received the name of Diana. The following elements have been computed from observations of March 15, 21, 25, and 31.

Epoch, 1863, March 31·5, Berlin m. t.

M	=	65° 44′ 54″·1
π	=	92 16 9 ·3
Ω	=	339 10 12 ·9
i	=	11 28 56 ·4
φ	=	13 12 13 ·0
μ	=	772″·860
log. a	=	0·441271

2. *Comet II*, 1863.—On the 12th of April, M. Klinkerfues discovered a new comet in R. A. 309° and Dec. 3° S. It was discovered independently by M. Donati at Florence on the 15th. On the 19th of May, this comet was only ten degrees distant from the North pole, presenting the appearance of a round nebulosity 5 or 6 minutes in diameter. The following elements have been computed by M. Loewy.

Perihelion passage, 1863, April 4·89477, Greenwich m. t.	
Longitude of perihelion,	255° 15′ 17″ mean eq. 1863·0
Longitude of node,	251 15 51
Inclination,	112 37 57
Log. perihelion distance,	0·026080

3. *Comet III*, 1863.—This comet was discovered by M. Respighi at Bologna, April 13th, near β Pegasi. The nucleus had the brightness of a star of the sixth magnitude, and the tail extended 40 minutes of arc. The same comet was also discovered about simultaneously by M. Becker, near Berlin, and by M. Tempel at Marseilles. On the 25th of April its tail was 2 degrees in length. M. Valz has communicated the following elements deduced from the observations of M. Tempel.

Perihelion passage, April 22·089, Marseilles m. t.	
Longitude of perihelion,	298° 58′
Longitude of node,	243 29
Inclination,	85 27
Perihelion distance,	0·6144
	Motion direct.

4. *Observations of the Zodiacal Light;* by Stillman Masterman.—The *zodiacal light* is one of the most difficult to observe, in our latitudes, of the celestial phenomena. Besides the unfavorable position or invisibility in the twilight, rendering it unobservable for a considerable portion

of the year, the moon and even the larger planets completely eclipse its mild light much of the time when it might otherwise be seen.

Since the commencement of 1859, I have been able to obtain a few observations of this body, mostly during the winter of 1861, which have been confined principally to the determination of the position of the *apex* of the luminous cone in the celestial sphere. My method of observation is as follows:—By means of a good star chart, I determine, as nearly as practicable, the *right ascension* and *declination* of the *vertex* or extremity of the axis of the cone; these coördinates being then easily convertible into those of *longitude* and *latitude*, thus giving the position referred to the ecliptic.

I have devoted very little attention to the physical phenomena of this body. Perhaps the phenomenon of rapid variations in brightness, observed more than once, may not be without interest. These are not wave-like pulsations like those observed in the aurora borealis; but the alternate brightenings and dimmings of the whole area of the light simultaneously. Sometimes a sudden brightening or dimming is observed without any other change following for a number of minutes. The latter was particularly observable on Jan. 9, last.

In the annexed small table of observations, the column headed $\lambda - \odot$ contains the difference between the observed longitude of the apex of the light, and that of the sun taken from an ephemeris; in other words, the angular distance of the vertex from the solar centre. That headed β shows the observed latitude of the vertex, north latitudes being regarded as positive.

Date.	$\lambda - \odot$	β
h	°	° ′
1859, Jan. 23, —	104·8	+ 2 49
26, 8·2	103·8	+ 3 17
31, 7·5	101·7	+ 3 31
1861, Jan. 4, 7·5	86·5	+ 0 38
9, 7·5	84·4	+ 2 38
Feb. 1, 7·2	72·9	− 2 14
3, 8·0	78·1	− 2 47
4, 8·5	74·8	− 3 3
Mar. 6, 8·0	70·8	+ 3 33
7, 8·5	79·7	+ 1 31
28, 8·5	72·4	− 2 8
Apr. 7, 8·5	71·9	− 0 28
1862, Dec. 11, 7·0	71·4	+ 3 17
1863, Jan. 9, 6·8	86·8	+ 3 7
18, 8·3	90·5	− 0 8

Weld, Franklin Co., Maine, Apr. 15, 1863.

5. *Results of Observations of Variable Stars at Weld, Franklin Co., Maine;* by Stillman Masterman.—I send you the results of my observations of variable stars since the commencement of the year. I had formed a design of observing all of those variables visible to the naked eye, also such as require only a small optical power for that purpose; but a long-continued illness, from which I now find myself far from being exempt, has limited me to those determinations annexed.

β Persei.

***Minimum*, 1863, Feb. 2, $7^h\ 24^m\ 48^s$, Wash. M. T.—Wt., ½.**

λ Tauri.

***Minimum*, 1863, Jan. 18, $10^h\ 23^m$, Wash. M. T.—Wt., 1.**

δ Cephei.

***Minimum*, 1863, Jan. 8, $11^h{\cdot}6$,—Weld, M. T.—Wt., 3.**
19, 7 ·1,— " 4.
Feb. 4, 8 ·5,— " 4.

ζ Geminorum.

Minimum.	*Maximum.*
1863, Mar. 23, 0^h—Wt., 1.	1863, Feb. 25, 7^h—Wt., 3.
Apr. 1, 15 " 2.	Mar. 17, 16 " 4.
12, 1 " 3.	
22, 6 " 4.	
May 1, 7 " 2.	

β Lyræ.

Principal Minimum.
1863, Jan. 1, $6^h{\cdot}3$—Wt., 4.

Maximum.
1863, Jan. 17, $5^h{\cdot}3$—Wt., 1.

Second Minimum.
1863, Jan. 19. $22^h{\cdot}8$—Wt., 1.

α Herculis.

Observations discontinued on this star, Feb. 15.
Approximate time of Maximum, 1863, Feb. 6.

Weld, May 30, 1863.

6. *Evidence of the cosmical origin of shooting stars derived from the dates of early star-showers.*—Mr. Quetelet, in his *Physique du Globe*, devotes a chapter to shooting stars. Doubts seem to have arisen in the mind of the distinguished author respecting the origin of these phenomena. Yet from this same chapter can be drawn a simple, and, as we think, a very strong argument, that the star-showers, at least, are caused by the entrance into our atmosphere of bodies revolving about the sun. And if this be admitted for the shooting stars of the annual periods, probably no one will deny that the sporadic meteors have a similar character and origin.

The return of the August and other showers on fixed days of the year might *possibly* be due to meteorological changes. But if the magnetism, the heat, the electricity, or the other properties of the atmosphere produce these *annual* phenomena, the period should evidently be the *tropical* year. On the other hand, if rings of bodies revolving about the sun are met by the earth in April, August, November, &c., thus causing these showers the cycle should be the *sidereal* year, or rather, should not be the tropical year. The nodes of the rings move along the ecliptic, but at a rate different from the precession of the equinoxes. The dates of the earlier showers show quite clearly that the true period is not widely different from the sidereal year.

In the following tables are given the historic dates of star-showers from

Quetelet's list, in some cases changed to agree with the authority cited. They are expressed in the Gregorian calendar, and therefore represent approximately corresponding dates of the tropical year. The later dates are generally omitted. To express these dates in a sidereal year there is given at the same time the *corresponding* day (and fraction of a day) of 1850; that is, the time when the earth's longitude in her orbit, measured from a fixed equinox, was the same as on the day of the shower. The following formula is used in the computation.

Let x be the number of days to be added to the recorded date expressed in the Gregorian calendar, t the given year of the Christian era, n the number of leap years between the given date and A.D. 1850, and l the length in days of the sidereal year. Then, evidently,

$$(1850-t)\,l=x+365\,(1850-t)+\mathrm{N}.$$

To reduce this to a form better suited for computation, observe that N is equal to the integral part of $\frac{1}{4}(1851-t)$, minus 12, plus the correction between the Gregorian and Julian calendars for the given date. Let c be this correction, ε be the remainder after dividing $1851-t$ by 4, $l=365^{\mathrm{d}}\cdot256374$, and we obtain, by reducing,

$$x=(1850-t)\times\cdot006374+\tfrac{1}{4}(\varepsilon-1)+12-c.$$

The integral part of $\frac{1899-t}{100}$, minus the integral part of $\frac{1999-t}{400}$, gives the value of $12-c$.

It will be observed that the secular variation in the value of l, the motion of the apsis of the earth's orbit, the diminution of its excentricity, and the periodic perturbations are neglected. The terms dependent on these would together rarely amount to one-tenth of a day. The equation of the center is therefore omitted. I have subtracted three-tenths from the Chinese dates, and added two-tenths to the American dates, for difference of longitude.

The noon of the historic date is taken when it is not stated whether the shower was in the morning or the evening. This involves an error not exceeding seven-tenths of a day. It should also be borne in mind that the shower in August (and probably those in other months) must be considered as continuing through more than a single day.

Particulars respecting the several showers are given in Quetelet's catalogue; also in Arago's *Astronomie Populaire*, iv, 292. In the following table, reference is made to the memoir of Ed. Biot, *Mémoires des Savans Etrangéres*, vol. x, Paris, 1848, to the paper of Chasles in the *Comptes Rendus*, xii, 499, and to Herrick's catalogue of star-showers, this Journal, [1], xl, 349. The dates reported by Biot are from the Chinese annals.

I. *The April shower.*—The following seem to belong to this period:[1]

B.C.	687, Mar.	16,	corr. to	A.D. 1850,	Apr.	19·9.	*Biot.*
	15, "	25,	"	"	"	19·6.	"
A.D.	582, "	31,	"	"	"	18·1.	*Chasles.*
	1093, Apr.	9·6,	"	"	"	20·7.	"
	1094, "	10,	"	"	"	20·8.	"

[1] Compare with these the following showers for which the exact date is not given:

A.D.	590, before Easter, or	Apr.	4,	corr. to	Apr.	22·1.	*Chasles.*
	741, "	"	13,	"	"	28·3.	"

A.D. 1095, Apr.	9·6,	corr. to	A.D. 1850,	Apr.	20·2.	*Herrick.*
1096, "	10,	"	"	"	21·3.	"
1122, "	10·6,	"	"	"	20·2.	"
1123, "	11,	"	"	"	20·4.	*Chasles.*
1803, "	19·6,	"	"	"	19·9.	*Herrick.*

II. *The August shower.*—To this belong the following dates:

A.D. 830, July	26,	corr. to	A.D. 1850,	Aug.	9·2.	*Biot.*
833, "	27,	"	"	"	10·4.	"
835, "	26,	"	"	"	8·9.	"
841, "	25,	"	"	"	8·4.	"
924, "	26–28,	"	"	"	8·1–10·1.	"
925, "	27, 28,	"	"	"	8·8, 9·8.	"
926, "	27,	"	"	"	8·6.	"
933, "	25–30,	"	"	"	5·8–10·8.	"
1243, Aug.	2,	"	"	"	10·6.	*Herrick.*
1451, "	5,	"	"	"	10·0.	*Biot.*[2]

III. *The November shower.*—The following appear to be exhibitions of this shower:[3]

A.D. 585, Oct.	25,	corr. to	A.D. 1850,	Nov.	12·3.	*Chasles.*
902, "	29 or 30,	"	"	"	11·0 or 12·0.	*Herrick.*
1582, Nov.	7,	"	"	"	10·7.	*Wartmann.*
1698, "	8·6,	"	"	"	11·6.	"
1799, "	11·6,	"	"	"	12·9.	*Humboldt.*
1833, "	12·7,	"	"	"	13·3.	*Olmsted.*

IV. *The December periods.*—There appear to be two epochs in December, each marking a distinct shower, viz: Dec. 6th–7th, and Dec. 12th. There is no early date corresponding to the first epoch. The following belong to the second:

A.D. 901, Nov.	30,	corr. to	A.D. 1850,	Dec.	13·3.	*Herrick.*
930, "	29,	"	"	"	11·6.	*Biot.*
1571, Dec.	8,	"	"	"	11·5.	*Wartmann.*

The remaining early dates of Quetelet's catalogue are arranged here for comparison with those already given. A few, which do not appear to indicate star-showers, are omitted. Many of those retained doubtless refer to auroras, or to moderate exhibitions of shooting stars.

January.

A.D. 599–600, Dec.	28,	corr. to	A.D. 1850,	Jan.	16·2.	*Biot.*
745, Jan.	5,	"	"	"	20·2.	*Perrey.*
765, "	3, 5,	"	"	"	18·2, 20·2.	*Biot* and *Perrey.*
848–849, Dec.	31,	"	"	"	14·1.	*Chasles.*
1118–1119, "	27,	"	"	"	5·4.	"

February.

A.D. 308, Jan.	20,	corr. to	A.D. 1850,	Feb.	9·0.	*Biot.*
913, Feb.	7,	"	"	"	20·2.	*Chasles.*
918, "	6,	"	"	"	18·9.	"
919, "	6,	"	"	"	18·6.	"
1106, "	19,	"	"	"	28·7.	*Herrick.*

[2] Biot gives July 27th, O. S., or Aug. 7th, N. S., which are not consistent. As the other dates are Old Style the former is presumed to be the correct day.

[3] A great shower occurred in the beginning of the year of the Hegira 596. This year began A.D. 1199, Oct. 30, which corresponds to Nov. 8·4.—*Herrick's Catalogue*, No. 28.

March.

A.D.	36, Feb.	7,	corr. to A.D. 1850, Mar.		2·1.	*Biot.*
	807, Mar.	2,	"	"	16·4.	*Chasles.*
	842, "	5,	"	"	19·4.	"
	842, "	17,	"	"	31·4.	"
	861, "	14,	"	"	28·6.	"
	937, Feb.	19,	"	"	4·1.	"
	1584, "	28,	"	"	3·2.	*Wartmann.*

April.

A.D.	401, Apr.	9,	corr. to A.D. 1850, Apr.		29·2.	*Biot.*
	538, "	6,	"	"	24·4.	*Chasles.*
	839, Mar.	29,	"	"	12·2.	"
	839, Apr.	17,	"	"	30·9.	*Biot.*
	840, "	1,	"	"	15·9.	*Chasles.*
	927, "	17,	"	"	29·3.	*Biot.*
	934, "	18,	"	"	30·8.	"
	1000, "	4,	"	"	15·9.	*Chasles.*
	1008, "	2,	"	"	13·6.	*Biot.*
	1009, "	16,	"	"	27·6.	*Chasles.*

May.

A.D.	839, May	12,	corr. to A.D. 1850, May		26·2.	*Chasles.*
	842, "	5,	"	"	19·4.	"
	954, "	11,	"	"	23·7.	"
	965, "	17,	"	"	29·7.	"
	1158, "	3,	"	"	12·4.	"

No shower in June.

July.

A.D.	36, June	25,	corr. to A.D. 1850, July		20·8.	*Biot.*
	784, July	14,	"	"	29·0	"
	1022, June	28–30,	"	"	9·3–11·3.	*Chasles.*

August.

A.D.	714, July	19,	corr. to A.D. 1850, Aug.		2·9.	*Biot.*
	865, Aug.	5,	"	"	19·3.	"

September.

A.D.	532, Aug.	30,	corr. to A.D. 1850, Sept.		17·6.	*Biot.*
	1012, Sept.	17,	"	"	28·5.	"
	1037, Aug.	27,	"	"	7·1.	"
	1063, "	28,	"	"	7·5.	"

October.

A.D.	288, Sept.	26,	corr. to A.D. 1850, Oct.		18·2.	*Biot.*
	585, "	25,	"	"	13·0.	"
	931, Oct.	19,	"	"	31·3.	"
	934, "	19,	"	"	31·5.	*Chasles.*
	945, Sept.	20,	"	"	3·0.	"
	1002, Oct.	20,	"	"	31·1.	*Biot.*
	1436, "	11,	"	"	16·8.	"
	1439, "	14,	"	"	19·1.	"
	1743, "	15,	"	"	16·4.	*Herrick.*
	1798, "	14·5,	"	"	15·8.	*Brandes.*

November.

A.D.	855, Oct.	21,	corr. to A.D. 1850, Nov.		4·1.	*Chasles.*
	856, "	21,	"	"	4·8.	"
	970, Nov.	8,	"	"	20·3.	*Biot.*
	979, "	2,	"	"	14·3.	*Chasles.*
	1058, "	7,	"	"	18·0.	"
	1101, Oct.	24,	"	"	3·0.	*Perrey.*
	1202, "	26,	"	"	4·1.	*Herrick.*
	1366, "	29·5,	"	"	5·6.	"
	1533, Nov.	3,	"	"	7·0.	*Biot.*

December.

A.D.	848, Dec.	1,	corr. to A.D. 1850, Dec.		15·9.	*Chasles.*
	899, Nov.	18,	"	"	1·8.	*Herrick.*
	1565, Dec.	13,	"	"	17·1.[4]	This Jour., [2], xxxv, 461.

The following dates are indicated by this table as deserving the attention of observers, viz: Jan. 15–19, Feb. 19, Mar. 1–4, Apr. 28–30, Oct. 16–18, and Oct. 31 to Nov. 6. During this last period have occurred several of the most remarkable showers on record. By giving to the nodes of the November ring a procession of one day in 70 years most of these would be brought into the November period.

For five of the dates given above, the day of the month depends upon the time of Easter. An error of the year, which might easily be made, would change the date. If for 538, 840, 1000, 1009, and 1158 we could read 536, 842, 1002, 1010, and 1160, the dates of the showers would correspond respectively to April 18·9, 20·4, 20·4, 19·4, and 18·9.

Yale College, June 16, 1863. H. A. NEWTON.

7. *The meteoric iron from Newstead.*—In the year 1827, whilst digging a cellar in the village of Newstead, Roxburghshire, Scotland, sticking in the clay at a depth of from 3 to 4 feet, the second and largest mass of meteoric iron yet found in Great Britain was discovered. Although its true nature had been overlooked, it has been preserved as a curiosity during 35 years, until it attracted the attention of Dr. John Alex. Smith, who read a paper on it at the meeting of the Royal Physical Society of Edinburgh, (April 23d, 1862) from which we learn the following particulars:

Its external surface is rough and irregular; its shape principally consisting of a large rounded and lobulated mass, which tapers rapidly at one end to a four-sided pyramidal extremity, and terminates in an obliquely truncated point. For a more detailed description it may be divided into two portions: the larger rounded extremity, and the smaller, flattened, smoother on its surface, and tapering to a blunt point. The larger portion terminates behind in a broad blunt edge, and is formed by a clustering mass of rounded lobes irregularly grouped together, variable in size and in their greater or less projection from its surface. A deep furrow runs obliquely round the whole mass, and towards the pointed extremity rises a large round prominent lobe with a smaller one on one side and two more irregularly shaped projecting masses on the other. The smaller portion of the meteorite lies immediately in front of the prominent lobes just described, its rounded outlines rapidly changing into

[4] I assume that the date, Dec. 3d, is old style. If not, the corrected date should be Dec. 7·1, and this belongs to the first December shower.

four irregular smoother planes, which, meeting one another with two acute and two obtuse angles, form a somewhat pyramidal four-sided figure, tapering rapidly towards this end of the mass, and terminating in an irregular quadrilateral extremity.

In its greatest length it measures 10¾ inches, and in its widest part, about the middle of its length, 7 inches; its circumference round the larger extremity is 1 foot 3 inches, in its widest part, round the lobular projections, 1 foot 8½ inches, while within 1½ inches of the point its circumference is only 9½ inches.

It weighed 32lbs., 11 ounces and 1½ drachms avoirdupois.

Its surface has a dark reddish brown, in some parts a blackish, color; the lobulated parts show here and there, especially in the furrows, spots of a brighter red color. For the purpose of preserving the original shape, plaster casts were taken before it was cut up.

It was cut longitudinally into two portions, and one of them again into smaller slices.

It was found to be entirely free from any foreign admixtures, such as olivine, etc., and of a bright white color, and solid, dense, and steel-like throughout. In attempting to file off a portion for analysis, the filings were black, and showed little of the metallic appearance of pure iron, the hands becoming much blackened, as if plumbago was mixed with the iron. It was extremely hard and tough, but the mass varied in its resistance to the file and a graving tool. The lobed and rounded portion resembled cast iron, but was harder than untempered steel of the best quality, though not so hard as the prepared steelplate of the engraver. The inner portions were softer and more open in texture than that next to its outer surface; the prism-like point was tougher and more like hammered iron. The mass was apparently not malleable but brittle. Etching by a mixture of nitric and acetic acids showed upon the surfaces the rough irregular projecting lines of the crystalline structure of the mass, which had been but slightly acted upon, the dark spaces and lines showing where the acids had the greatest effect. A small patch near the middle of the rounded or lobed portion etched with dilute nitric acid has a similar appearance, but displays more distinctly the characteristic and frosted-like lines of crystallization crossing each other at various angles. These lines are very fine and minute in texture, and the meteorite resembles in structure that of hazelnut size found many years ago at Leadhills, and described by R. P. Greg, Esq.

Dr. Murray Thomson found the spec. grav. of different portions, 6·1919, 6·499 and 6·7400; that of the pyramidal portion 6·750; that of the lobed portion 6·350, that of the whole mass =6·517. The composition of the meteorite is according to Dr. M. Thomson:

Iron, - - -	93·51
Nickel, - - -	4·86
Silica, - - -	0·91
Carbon, - - -	0·59
	98·87

8. *The meteoric iron from Sarepta.*—Director Wm. Haidinger made at the meeting of July 24th, 1862, of the Imperial Academy of Vienna, some interesting observations on the meteoric iron from Sarepta. His paper is accompanied by two plates, one showing the peculiar appearance of the

Sarepta iron in three positions, the other representing prints from a galvanoplastic copperplate, prepared from the etched slices of Sarepta and Arva iron, showing their structure; together with two prints from the etched plates themselves.

Although it is very difficult to form without these illustrations a correct idea of the appearance and structure of this meteorite, we will give the following abstracts:

It was found in 1854 on the right bank of the river Volga in the steppes of the Kalmucs, 30 miles (German) from Sarepta in the district of Zarizin, Govt. Saratow in Russia. Its original weight was 32lbs. 58 zolotnik, =31·58 lbs. avoirdupois or 14325 grs. The first notice of it was given at the meeting of Nov. 18th, 1854, of the Imp. Soc. of Naturalists of Moscow, by Dr. Auerbach, who exhibited it for Constantine Glitsch of Sarepta, at whose direction plaster casts were made of the mass, the original however being cut to pieces for distribution.

It is a compact iron mass, pretty rich in nickel, rounded at the edges, and entirely free from olivine or any other foreign substances. The Sarepta meteorite is one of the most remarkable and peculiar known; a most characteristic difference, distinguishing its two sides, can readily be perceived. The front side has the form of a gently sloping arch, similar to a spherical surface, the radius of which is about 9½ inches. The roundish depressions upon it are only from 1 to 3 lines in depth, their diameter being from 1 to 1½ inches; the back part, on the contrary, is full of rounded indentations, excavated to a depth of 1½ inches, the formation of which can be easily imagined to have been produced by the melting off by pointed flames uniting backwards. The model does not show well the characteristic sharply turned up ridges produced by the melting of the crust.

From all these data there seems to be no doubt that the position here suggested was really that which the meteorite had during the cosmical part of its path. The centre of gravity lies certainly nearer to the flat spherical convexity than inside of the ruggedly juxtaposed cones and excavations, notwithstanding the flat mass which projects here with its broad plane. The stone of Stannern, which Haidinger described a short time ago (*this Journal*, [2], xxxii, 138), has a similar disc-like shape; but the large mass from Agram of a flat disc-like shape, would answer still better for comparison, the two sides of which are so different, that with the greatest probability we might infer that it had a rotation in its plane round the axis of its orbit.

The Sarepta iron does not appear to have been very long exposed to the atmospheric influences, its surface being hardly acted upon by rust. It presents in its structure many analogies with that of Arva. The schreibersite in the figures upon the plates of the Sarepta iron does not show any interruption in its direction, although it does not penetrate them uniformly. The meteoric iron immediately adjoining the schreibersite shows distinctly fine characteristic striæ and hatchings, most plainly in the darkest portions, and through them a damasked appearance, if examined with a magnifier. The different parts on account of the different reflexion of light appear paler or darker grey, some even quite dark and without lustre, but, cut in a different direction just these portions would present more lustre and damask, and others which are now the most

brilliant would show it less. Yet these padded particles which produce such different impressions do not appear to be of different nature, their position only in the compound causes the different appearance. The Sarepta plate consists, in one of its corners, of meteoric iron only without any schreibersite, but of two peculiar varieties; one is pale gray, smooth, brilliant and full of striæ and hatchings, intersecting at right and oblique angles, nearly as a square and its two diagonals; the other is quite dull, dark gray, more granular in texture and abruptly separated from the other. Held towards the light in very oblique angles, it shows a paler gray color, and a little lustre caused by the damask. Scattered through the dull dark mass are very minute brighter shining particles.

The Arva plate shows the intersections of the brilliant schreibersite in three directions at angles of 60 and 120°, the iron being dull and dark gray without striæ; in one corner there is some graphite, inclosing a roundish parcel of sulphid of iron. The plates from Arva iron, however, present great differences amongst themselves. F. A. Gth.

9. *Meteoric Iron from Tucson, Arizona.*—A mass of meteoric iron from Tucson has been presented to the city of San Francisco by General Carleton. In a recent letter, Prof. Whitney states that this iron is 4 feet 1 inch long, and weighs 632 lbs. It was found at or near Tucson, Arizona, by Gen. Carleton's California column on their march through that region, and has evidently been used for an anvil, although it is not the one figured by Bartlett as having served that purpose.

A specimen of this meteorite was sent to Prof. Brush, of Yale College, for chemical investigation, and we condense the following account of this examination from a letter addressed to Prof. Whitney and published in vol. iii. of the *Proceedings of the California Academy of Natural Sciences:* "An inspection of the specimen with a lens showed it to be dotted with little cavities which, on the fresh fracture, were lined with a white silicious mineral, giving the surface a porphyritic, or pseudo-porphyritic, appearance. Its specific gravity is 7·29. When a fragment of it was placed in a solution of neutral sulphate of copper, it became quickly coated with metallic copper, proving the iron to be 'active.' Attacked with an acid, a portion of the iron was dissolved, leaving the silicious mineral projecting from the surface of the specimen; and, with a magnifier, black particles of schreibersite could be seen. After complete solution of the iron, a careful microscopic examination was made of the insoluble residue. With a magnifying power of 25 diameters, it appeared to consist chiefly of two substances: one a milk-white to transparent mineral, having a fused, rounded surface, occurring in little globules, or elongated, rounded particles; while the other constituent was black and angular, and attractable by the magnet. The first named substance, when observed with a magnifying power of 100 diameters, proved to contain minute specks of the black mineral disseminated through it; some of the silicious fragments were translucent and of a milk-white color, and others colorless and transparent; a large number, however, were transparent at one end, shading into milk-white at the other, thus seeming to indicate that the transparent and translucent portions were not two distinct minerals. A blowpipe examination of the silicious mineral showed it to have characters very much resembling *olivine*. The black mineral proved to be *schreibersite*. A minute trace of chromium was also observed in the insoluble residue.

The qualitative analysis of the portion soluble in nitric acid indicated the presence of iron, nickel, cobalt, copper, phosphorus, lime, and magnesia, with unweighable traces of chlorine, sulphur, and alumina. For the quantitative examination of the meteorite, a fragment weighing 4·3767 grammes was treated with nitro-chlorhydric acid (aqua regia), and after solution of the iron the whole was evaporated: on approaching dryness, gelatinous silica separated, showing that the silicate had been partially, at least, decomposed by the acid. After heating until the silica was rendered insoluble, it was repeatedly treated with acid and evaporated, so as to insure the oxydation of all the schreibersite, and finally the soluble part was taken up with chlorhydric acid, and on dilution separated by filtration from the silica and insoluble residue.

The *insoluble residue*, containing free silica and undecomposed silicate, was perfectly white and free from all traces of schreibersite. It weighed 0·1855 grm., equal to 4·24 per cent of the specimen analyzed. It was fused with carbonate of soda, and the silica and bases determined in the usual manner. It contained 0·159 grm. silica; 0·0054 protoxyd of iron, with a minute trace of alumina; 0·0028 lime, and 0·0168 magnesia.

The soluble and insoluble portions gave in the analysis the following percentage composition:

			Considering the silica to exist as olivine.
Iron	81·56		79·44
Nickel	9·17		9·17
Cobalt	0·44		0·44
Copper	0·08		0·08
Phosphorus	0·49		0·49
Silica	3·63		
Protoxyd of iron, with a trace of alumina	0·12	Combined with 2·73 Protoxyd of iron, making olivine	10·07
Lime	1·16		
Magnesia	2·43		
Chlorine, Sulphur, Chromium	minute traces		traces.
	99·08		99·69

If the silica found in this analysis be considered to exist in combination with lime, magnesia, and iron, in the proportions to form olivine, it will be necessary to deduct 2·12 per cent from the amount of metallic iron (equal to 2·73 per cent of protoxyd of iron), in order to give the silicate the olivine formula ($3RO, SiO_3$). Admitting this to be the correct view, the mass analyzed contains 10·07 per cent of olivine, and by the addition of the oxygen of the protoxyd of iron the analysis adds up 99·69 instead of 99·08.

The composition of this meteorite corresponds very closely with another meteoric iron from Tucson, discovered by Mr. Bartlett, and described by Prof. J. Lawrence Smith, in the *Am. Journ. of Science*, 2d ser., vol. xix, page 161. Dr. Smith's analysis gives iron 85·54, nickel 8·55, cobalt 0·61, copper 0·03, phosphorus 0·12, chromic oxyd 0·21, magnesia 2·04, silica 3·02, alumina trace, =100·18. He considers it to correspond to nickeliferous iron 93·81, chrome iron 0·41, schreibersite 0·84, olivine 5·06 =100·18. By an evident inadvertence, Dr. Smith adds the magnesia

and silica together, and gives the sum as olivine; these substances are obviously not in the proportions to form the silicate 3RO, SiO_3, and if we consider the silicate to be olivine, we must reckon the excess of silica as combined with protoxyd of iron. To do this, we must deduct 2·83 from the amount of metallic iron (equal to 3·64 protoxyd of iron), necessary to be combined with the silica and magnesia to give the olivine formula. The amount of olivine contained in the Bartlett meteoric iron will then be 8·70 per cent. Thus the two masses of iron will be seen to agree very nearly in composition, the only trifling difference being, that Dr. Smith has determined quantitatively the small amount of chromium contained in the Bartlett meteorite, while I have found a little lime and traces of sulphur and chlorine in the specimen you sent to me. The specific gravity I have stated to be 7·29; this was taken on about 12·5 grammes of the iron, and probably is somewhat higher than the portion which I analyzed, as the two surfaces of the larger specimen had been rubbed down, and as thus a considerable portion of the exposed silicate would be mechanically removed, it would make the density correspondingly higher." G. J. B.

10. *Meteor of April 19th seen at Philadelphia.*—A brilliant meteor was seen at Philadelphia and vicinity, on Sunday evening, April 19th, at 10 minutes before 8 o'clock.

Its apparent size was rather less than that of the full moon, its brilliancy considerably greater, form globular, and direction of motion nearly from west to east, tending slightly southward.

It seems probable that its first appearance was over the eastern edge of Chester County, although the data are not sufficient to determine satisfactorily either the place of beginning or the velocity. It is very clearly proved, however, by comparing observations made at Philadelphia, West Town (4 miles east of West Chester), Wilmington and Odessa, Del., that it disappeared over the western part of Camden County, N. J., (probably about 5 miles north of Glassboro), at a height of between 12 and 20 miles.

The duration of visibility was estimated at from 3 to 6 seconds.

Both at Wilmington and West Town, it was followed, after an interval of about 3 minutes, by a noise like thunder. At the former place, a gentleman who did not see the meteor compared the sound to that of a cannon at Fort Delaware, ten miles distant.

VI. SCIENTIFIC CORRESPONDENCE.—*Observations on Stellar Spectra.*

EDITORS OF THE AM. JOURNAL, &c.

Gentlemen: During the past year several European astronomers have been occupied with the observation of stellar spectra. I have now before me the results obtained by Donati,[1] Airy,[2] and Secchi.[3] I will at present confine myself to a short notice of the forms of instruments used by each as described in their published notices. Donati used a large burning glass, fifteen inches aperture and sixty-two inches focal distance, not achromatised, mounted equatorially, the cone of light passed through a fine slit before reaching the focal point; after crossing at the focus it traversed a cylindrical lens, thence, after being rendered parallel by an achromatic lens, fell upon a flint glass prism of about 61°, emerging from

[1] *Monthly Notices*, January 9, 1863. [2] Ib., April 10, 1863. [3] *Astr. Nach.*, 1405.

which it was received by a small achromatic objective, and observed with an eye-piece magnifying twelve times, in the focus of which was placed a bar movable by a micrometer screw.

From my experience in the observations of stellar spectra, I should note several defects in this arrangement. 1st. The absence of achromatism in the great condenser, in consequence of which but a small portion of the spectrum can at any time be brought to an approximate focus, and fine definition of the lines nowhere obtained.

2d. Too much power in the observing telescope, the objections to which are manifest.

3d. The uncertainty of making a contact of the micrometer bar with the lines, there being no illumination.

4th. The want of a check, such as the presence of a flame line in the field of view, to insure the detection of such small displacements of the spectrum as even a fine slit will permit.

The results of these instrumental defects will I think be seen when we hereafter examine the observations.

The Astronomer Royal describes the form of spectroscope used at Greenwich, where it is attached to the eye-tube of the great equatorial of twelve inches aperture. He says "The pencil of light from the object glass, which has converged to form the image of the star, then diverges, and falls in a wide and divergent state upon the prism; after emergence it is received on a combination of lenses which causes the pencils for the different colors to converge."

The image is observed with a micrometer, the field being illuminated by an annular reflector. In this form neither slit nor cylindrical lens is used, but breadth is given to the spectrum of the star by the creation of aberrations in two ways; first, by placing the prism in a position not that of least deviation, and second, by the uncorrected state of the "combination of lenses" through which the light reaches the micrometer after traversing the prism.

Reference is made to the lines of the solar spectrum in the following manner: A diaphragm, pierced by a hole $\frac{1}{30}$ of an inch in diameter, is placed in the focus of the great objective; when turned upon the sun, this hole represents the image of a "star of solar matter," and its lines are measured by the micrometer; an eye-piece containing a wire of reference is inserted with a reflector in a lateral tube and the position of the small hole observed. At night, the diaphragm is removed, and the image of the star is made to occupy the same position when seen in the lateral eye-piece occupied by the sun-illuminated hole, the eye-piece and reflector are then removed, and the observation made by the micrometer upon the star striæ.

Mr. Airy, in his description, speaks of this form of spectroscope as experimental, and expresses some doubts of its ability to define with great sharpness. This defect must necessarily result from the construction of the instrument, since the lines are only rendered visible by the existence of aberrations, which are destructive of fine definition. The spectrum not being confined during the observation to any certain part of the field of view, by a slit or other check, the truth of the measures depends entirely upon the exact running of the equatorial driving clock, which is not to be trusted. The illumination of the field must necessarily obliterate the

extremities of faint spectra, and render the observation of many lines impossible.

Secchi's spectroscope is on the whole much better than either of the former. The light of the great Roman equatorial passes through a slit at the focal point, then traverses a cylindrical lens, then is rendered parallel by a lens, and then falls upon the prism, which is a compound structure composed of four flint glass prisms of 90° cemented to five crown glass prisms of the same angle, and so arranged that the axis of the spectrum or central green ray is a prolongation of the incident pencil: the amount of dispersion obtained by this form is the excess of the four flint prisms over the five crown, and with ordinary materials will amount to about the dispersion of two 65° prisms of flint glass. This dispersion is so great that the lines in the stellar spectra are seen without an observing telescope. They are referred for measurement to an illuminated scale reflected from the last surface of the prism. This scale is made by fine perforations in a metallic plate. The instrument is provided with a reflector for the purpose of comparing the star lines with those of a flame.

The only objection I can see to this form of instrument is the great thickness of glass in the prisms, the consequent absorption of light, and the increased danger of heterogeneity of material. Its great advantages are that it reaches the separation of the lines by the dispersion of the prisms rather than by the power of an observing telescope, and that it possesses the means of constant reference to a standard flame to check any deviation in the place of the spectrum.

The advantage first named is much greater than would at first be supposed, as I have recently proved by the substitution of a bisulphid of carbon prism for the flint glass formerly used in my star spectroscope. With an eye-piece of one half of the power formerly used, I have now a spectrum longer than before with more than double the intensity of light, and a consequent revelation of striæ not before seen.

I have perhaps trespassed too much upon your space in this note upon the instrumental agency so far brought to bear upon the spectral analysis of the stars. But these investigations are yet in the cradle, and if, as is predicted, they are destined to assume a great importance in the study of the constitution of the universe, it will not be amiss to point out to those about to embark in the new field of labor, the advantages and defects of the star spectroscope as it at present stands.

In the April No. of the *Monthly Notices*, Mr. Glaisher describes observations on the length of the spectrum obtained from the sky at different altitudes, made by him during his balloon ascension on the 31st of March last.

At the surface he saw the spectrum from B to beyond G, and all the principal lines. The length of the spectrum gradually decreased until at the altitude of 4½ miles he had no spectrum. He says, "The shortening of the spectrum with increase of elevation may have been, and most likely was, owing to want of light (although to my senses there was abundance); the sky at the time was of a deep dark blue, the sun was low and it is possible the light was insufficient."

The shortening of the spectrum observed by Mr. Glaisher is without doubt due to the diminution of light and to the monochromatic nature

of the blue sky, which at that great altitude held but little vapor in suspension capable of reflecting white light. It is to be regretted that he did not make a series of observations upon the spectrum obtained directly from the sun. Such a series would have been of great value in determining the agency of our atmosphere in producing lines in the spectrum; and it is very necessary for the precision of astronomical chemistry to determine accurately which lines are telluric, and which are attributable to a celestial origin. You will remember that in my note to you of April last, contained in the May No. of this Journal, I sent you a diagram of the nine lines of which I found the solar D to be composed. I have since that date satisfied myself that of this group four only are truly solar lines and five telluric. Of this nature are the three faint lines on the red side of Kirchhoff's central line and the two faint ones next adjoining it on the green side. My proof is that these lines, although difficult objects at noonday with a battery of eleven prisms, are seen with ease near sunset with two. The whole of the yellow region of the spectrum is crowded with telluric lines, and it is most desirable that they should be accurately known. I am very respectfully yours, &c.,

LEWIS M. RUTHERFURD.

New York, June 8, 1863.

VII. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *On a System of Mounting Insects for the Microscope;* by HENRY T. VICKERS, B.A. (From the *Journal of the Royal Dublin Society*, No. 24, p. 271.)—The insects, after being caught, are put to steep in a solution of caustic potash, until they become clear, or nearly so; the strength of the solution which I think the best for general purposes is, half a drachm to the ounce. If stronger than this is used, I find it acts on the wings and delicate parts before the interior is sufficiently dissolved.

The next step is to place the insect in water, to wash away the potash. I generally allow it to remain twenty-four hours in water; I then take the slide on which it is finally to be mounted, and having rubbed it well to clean it, I float the insect on to the end of the slide, and not the middle (the reason for this will presently appear). I then place the insect as nearly as possible in the form in which I wish it finally to appear; and, taking another slide, at the end of which I hold a small bit of blotting paper, I lay it over the slide which has the object on it, the blotting paper being over and next the object; then gently and gradually press the two slides together, and pass over them a small clip made of flat brass-wire, and put the entire arrangement in water, to remain for twelve hours, at least.

Then take off the clip, turn the blotting paper which projects down over the slide which has the object on it, slip the other one off, and then gently peel off the blotting-paper, and in nine cases out of ten the object will adhere to the glass. Should you find the object sticking to the blotting-paper when you begin to peel it off, then try another corner, or even a third, before using a needle to it,—the great object being to use the needle in touching the insect as little as possible. Having got the blotting-paper off, and along with it most of the dirt, the next thing is to move the object to a clean part of the slide; and as the object is at the

end of it, you can give the centre an extra rub before moving the insect into it. The moving is done, not with the needle directly, but by dropping water on the slide from a pipette; and you will find that the insect will float on the top of the water, and can be easily guided to the centre of the slip; a touch of the needle will retain it there, and the water can be drained off. And now the eye-glass or dissecting microscope must be brought into action, and the object finally set out. The parts are still limber, and can be arranged, which they could not be if the objects were soaked in turpentine to make them clear, in the first instance, as they used to be. The object, being arranged, must be left to dry.

The most convenient way I find to be, to tie up a number of slides together, with a bit of cork between each: I think it better not to leave them too long to dry, as the turpentine penetrates more easily if they are not too dry. I then put the entire bundle into a large, wide-mouthed bottle of turpentine, and the longer it is left there the bettter.

When proceeding to mount, I select a number of covering glasses, such as are likely to suit one of the bundles, and, after cleaning, place them on a long brass plate over a gas jet, and dropping as much Canada balsam as will just fit on the covers, and applying as much heat as will enable me to skim off the bubbles with a needle, I allow the covers and balsam to get *cold;* I then take one of the covers with a small forceps, and plunging it into a vessel of turpentine, apply it at once (balsam downwards, of course), to the object on the slide, slip on another brass clip, which is made so as to touch the cover only in one point, and that point the centre, then lay it on a little tin tray: when all the bundle or bundles are thus mounted and placed on the tray, I put the tray itself in a slow oven, and leave it there till all the turpentine is pressed out by the spring clip under the action of the low degree of heat. If too much heat be applied, the turpentine in the inside of the animal will boil, and bubbles will be produced, which you will find it very difficult or impossible to get rid of, and the object is spoiled.

It is in this part of the mounting that I consider my system has a great advantage over others which I have heard and read of. Some use balsam thinned with chloroform; when such is used before the specimens are perfectly dry, they are easily displaced, and in some instances entirely rubbed off the slide. I have heard of this having happened with several English mounted objects; and I myself have seen, in one of these which I have, the fluid mass moving in the inside, and running about among the legs of the animal, proving to me that I must be careful how I clean it; and I fancy that when the balsam does dry, it becomes very brittle. Mounting the way I do, when the slides have become clear and free from streaks in the oven, they are quite hard, and may be at once cleaned off with benzole and washing-soda.

2. *Collection of Minerals and Chemical Apparatus belonging to the late Prof. Manross.*[1]—This collection consists of about 500 good specimens of rocks and minerals, together with many hundreds of smaller size.

[1 It will be remembered that Professor (Captain) Manross fell at the battle of Antietam, Sept. 17, 1862, while leading a charge. It would be a most worthy act of recognition of his patriotic devotion to purchase his instruments and collections for the benefit of his widow.]

They are mostly from New England, and embrace several large lumps of the Haddam chrysoberyl rock now no longer to be obtained. Many valuable specimens of gold and sulphur from South America and Mexico are also embraced in the collection. The entire cabinet has been appraised at the moderate sum of $100.

Among the apparatus are balances for analysis, a reflecting goniometer of the most finished and perfect German construction, and platinum and silver crucibles. The whole may be viewed at any time at the residence of Mrs. Manross, at Forrestville, Conn., or information respecting them may be obtained by application to Prof. C. U. Shepard (one of the appraisers of the estate) at Amherst College, Mass.

3. *Transactions of the Academy of Sciences of St. Louis*, Vol. II, No. 1. 218 pp., 8vo, with several plates. 1863.—This number contains many valuable papers, among which are the following: on Botany and Meteorology (noticed on page 128, this vol.), by G. Engelmann; on Geology and Paleontology, by B. F. Shumard, G. C. Swallow, and H. Engelmann; on Atmospheric electricity, by A. Wislizenus; on the Ascent of Pike's Peak, by Dr. C. C. Parry.

4. *Insecteans.*—On page 7, in mentioning names for the subdivisions of Insecteans, the word for the *second* division is written *Octopods.* As this is identical with the name for a group of Cephalopods, it would be better to substitute the equally, or even more, correct form of the word, *Octapods.*—J. D. D.

5. *Officers of the American Academy of Arts and Sciences*, chosen May 26, 1863.—*President*, Asa Gray. *Vice-President*, Charles Beck. *Corresponding Secretary*, William B. Rogers. *Librarian*, Josiah P. Cooke. *Treasurer*, Edward Wigglesworth. *Council*, The President, Vice-President, and the Secretaries, *ex officio*, Thomas Hill, George P. Bond, John B. Henck, A. A. Gould, Louis Agassiz, Jeffries Wyman, Robert C. Winthrop, George E. Ellis, Henry W. Torrey. *Rumford Committee*, Joseph Lovering, Morrill Wyman, William B. Rogers, Joseph Winloch, Charles W. Eliot, Theophilus Parsons, Cyrus M. Warren. *Finance Committee*, The President and Treasurer, *ex officio*, J. Ingersoll Bowditch. *Publication Committee*, Joseph Lovering, Jeffries Wyman, Charles Beck. *Library Committee*, A. A. Gould, William P. Dexter, J. B. Henck. *Auditing Committee*, Thomas T. Bouvé, Charles E. Ware.

Book Notice.—

6. *Chauvenet's Spherical and Practical Astronomy.*[1]—In this work we have fresh evidence of the success with which astronomy has been cultivated of late years in the United States. The want, long ago felt, of some treatise on Practical Astronomy more modern and accessible than the bulky volumes of Pearson, was first met by the work of Prof. Loomis, published in 1855, which was at once adopted in the Universities of

[1] *A Manual of Spherical and Practical Astronomy:* embracing the General Problems of Spherical Astronomy, the Special Applications to Nautical Astronomy, and the Theory and Use of fixed and portable Astronomical Instruments. With an Appendix on the Method of Least Squares. By William Chauvenet, Professor of Mathematics and Astronomy in Washington University, Saint Louis, (Mo.) Vol. I, Spherical Astronomy. Vol. II, Practical Astronomy. Philadelphia: J. B. Lippincott & Co. London: Trubner & Co. 1863. Pp. 708, 632, 8vo.

Cambridge and Edinburgh as a text-book, and commended by leading English astronomers as the best in the language. The success of that work leads us to anticipate for Prof. Chauvenet's masterly and more extended treatise a reception, abroad as well as at home, every way worthy of its rare merits, and honorable in the highest degree to American science. It is fitting that, as we have already given to Practical Astronomy some of its most original and important improvements—as, for example, the American, or chronographic, method of transits, the telegraphic method of longitude, and Talcott's method of latitude—so we should also give to it a treatise of corresponding importance—such an one, in fact, as the work before us—the most complete and thorough that has yet appeared in any country or language.

Reserving for a following number of this Journal a more elaborate review of these volumes, we can here only indicate in brief their scope and leading features. The first volume, on Spherical Astronomy, discusses, with almost exhaustive completeness, the questions of parallax, refraction, time, latitude and longitude, eclipses, aberration, astronomical constants, etc. These discussions are characterized throughout by that remarkable generality and mathematical rigor, which belong distinctively to the German methods of investigation, so successfully employed by Bessel, and others of his school. Based upon these methods, Prof. Chauvenet's work represents astronomy in its most modern and perfected forms of research. Many of the investigations are, either wholly or in part, original—such, for example, as of some of the formulæ for latitude and eclipses, occultations of planets, improved methods of lunars, &c.

The same completeness and rigor of analysis characterize also the second volume, which embraces Practical Astronomy. The Theory of Instruments is particularly elaborate and exhaustive. Almost every conceivable use of each for any important purpose is carefully discussed. The chief topics of the volume are telescopes, the general theory of instruments for angles, instruments for time, the sextant, transit instrument, meridian circle, altitude and azimuth instrument, zenith telescope, equatorial telescope, heliometer, and the filar and ring micrometers. Old instruments and old methods are wholly discarded.

Not the least valuable part of the work is the Appendix of a hundred pages on the Method of Least Squares and Pierce's Criterion—examples of the application of which in the discussion of observations abound throughout the work. A few auxiliary tables—altogether too few—are given at the close, nearly half of them belonging to the author's method of lunars. The steel plates illustrative of instruments, though on a small scale, are very clear, and perhaps sufficiently in detail for the purpose they were intended to serve. The mechanical execution of the work—type, paper, &c.—are worthy of its scientific merits, and all that the most fastidious could desire.

The Computation of Orbits and Perturbations—topics, in part at least, belonging to practical astronomy, and naturally looked for in a work like this—obviously could not have been included without too great condensation or too great bulk, and are doubtless reserved by the author for another volume, which we trust the success of the present publication will induce him to prepare at an early day.